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COSMIC RAYS: RECENT DEVELOPMENTS

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§ 1. INTRODUCTION

RECENT developments of cosmic-ray theory and experiment have brought into marked prominence the problem of the energy spectra of the various sorts of rays. Till lately the dominant question was the nature of the particles; now that more is known of their nature, interest turns to the numbers of rays of given energy.

It is first necessary to distinguish between the various types of rays. At and below sea level, 80 per cent or more of the rays consist of about equal numbers of positively and negatively charged mesons ; their mass is about 170 times that of an electron and they decay spontaneously into electrons of the same sign, with a mean life when at rest of about 3×10^{-6} second. In view of their instability these mesons cannot have come from outside the atmosphere, but must be produced in the upper levels of the atmosphere, by some type of stable rays. The nature of these primary meson-producing rays is not yet certain (see § 5).

The main intensity of cosmic rays at high altitudes consists of extensive cascade showers of electrons of both signs together with photons derived, by the repeated process of emission of photons and pair production, from electrons incident on the atmosphere.

The mesons are everywhere accompanied by secondary cascade showers produced (*a*) by the electrons into which the mesons decay, (*b*) by the elastic collision of mesons with free electrons (§ 2.4), and possibly, (*c*) by the radiation loss of mesons during nuclear collisions.

At sea level there is a proton component, exclusively positive, amounting to about 5 per cent of the meson intensity (see § 2.5).

§ 2. MESONS AND PROTONS

§ 2.1. *The Absorption of Mesons*

The early experiments on cosmic rays showed that the *soft* component is absorbed roughly exponentially in the lower part of the atmosphere, that is to say, the intensity I varies with the thickness of the absorber h according to the relation

$$I \propto e^{-\mu H}, \qquad \ldots\ldots(1)$$

where μ is a constant.

When attempts were made to fit the observed absorption of the *penetrating* rays to the same expression it was found that μ was not all constant, but decreased as h increased. The first indication of a more suitable form of absorption law was given by Medicus (1932), who measured the variation of intensity I with zenith angle θ and found that I varies as $\cos^2\theta$. But it was not till the work of Follett and Crawshaw (1936) that it was recognized that the number N of the penetrating rays could be represented over a large range by a power law of the form

$$N(h) \propto h^{-n}, \qquad \ldots\ldots(2)$$

where n is about 2·0. The results of various workers as plotted by Blackett (1937 b) showed that the intensity variation, from 40 to 1500 metres of water equivalent, could be represented by (2) with $n = 1{\cdot}90$, but that n fell markedly towards smaller thicknesses of absorber. Ehmert (1937) made an extensive series of measurements in water and found $n = 1{\cdot}87$ for absorption thicknesses between 40 and 1000 m. of water. Clay (1939) found the value $n = 1{\cdot}92$ for absorption thicknesses between 60 m. and 450 m. H_2O, but for greater depths, from 400 m. to 1350 m., the value 2·92 (Clay and van Gemert, 1931). Wilson (1938) finds the value 2·52.

There is thus some uncertainty as to the exact law of absorption, especially at great depths, but it is clearly roughly of the power-law form, with an exponent which, if not constant, does not vary very fast. For the purposes of the subsequent discussion, we will assume a constant exponent of 1·9.

The importance of the law of absorption (2) lies in the fact that it can be immediately related to the energy spectrum of the mesons. On the assumption that the energy loss is independent of energy (Blackett, 1937a), the absorption law (2) indicates that the number $G(E)$ of rays of energy greater than E is of the form

$$G(E) \propto E^{-n}. \qquad \ldots\ldots(3)$$

Thus the fact that (2) holds roughly for 40 m.$<h<$1500 m. of water shows, on the basis of a constant energy loss of 2×10^6 ev. per cm. of water, that the energy spectrum of the mesons is roughly of the form (3) between 8×10^9 and 3×10^{11} ev.

It is often convenient to use not the integral spectrum $G(E)$ but the differential spectrum $g(E)dE$, that is, the number of rays with energy between E and $E+dE$. For the case of (3) we have

$$g(E) \propto E^{-(n+1)} \qquad \ldots\ldots(4)$$

and so, for $n = 1{\cdot}90$,

$$g(E) \propto E^{-2{\cdot}9}. \qquad \ldots\ldots(4\,a)$$

§ 2.2. *Effect of the Decay of the Mesons*

Experiments with solid absorbers (Alocco, 1935 ; Street and Woodward, 1935) showed that the absorption of the penetrating component depends almost exclusively on the mass of the absorber traversed and very little on the atomic number of the absorber (mass-absorption law). However, when a solid absorber is compared with a gaseous one (air), this is no longer the case ; on the contrary, a given mass of air absorbs the penetrating component more than the same mass of a solid material. This seems to have been first clearly established by measurements of Benedetti (1934), who showed that the intensity of the penetrating component under 12 cm. Pb was 11 per cent greater than under the same mass of air.

The same type of anomaly was clearly found by Follett and Crawshaw (1936) in their measurements, which have already been referred to. The absorption in air was found by measuring at sea level the variation of intensity with the zenith angle. The absorption in earth (London clay) was found by repeating the observations underground. The results showed that the intensity under a large thickness of air was less than one-half that under an equivalent thickness of earth. Ehmert (1937) found the same result by comparing the absorption in air with that in water. Auger, Ehrenfest, Fréon and Fournier (1937) observed the absorption in air by the zenith-angle method, at several heights above sea level and also underground, and found that the absorption by air at low density was greater than that at high density.

Kuhlenkampff (1938), following Yukawa's suggestion that the meson might be unstable, suggested that this instability would result in an additional absorption of mesons in gaseous absorbers due to their decay into electrons. In solid material the mesons are brought to rest so rapidly by ionization, and in consequence their decay so seldom occurs, that no appreciable extra absorption takes place.

§ 2.3. *The Theoretical Energy Spectrum of the Mesons*

Since it follows from the principle of relativity that the mean life of a meson must be proportional to its momentum, the rays of low energy will be more absorbed in the atmosphere than those of high energy.

Heisenberg and Euler (1938) have given an explanation of the energy spectrum of the mesons on this basis. They assume that the mesons are formed in the upper atmosphere with a spectrum of the form (4) with $n=1{\cdot}87$. At sea level this distribution is maintained nearly unchanged for the high-energy rays, but those of lower energy suffer additional absorption by decay, so that the spectrum falls off in intensity at low energies relative to the expression (4).

The differential energy spectrum of the penetrating rays at sea level has been measured over the range of energy from about 10^8 ev. to 2×10^{10} ev. by Blackett (1937a) and by Wilson,* and is found to agree closely with the spectrum

* J. G. Wilson. Unpublished.

to be expected, assuming that the mesons are created in the upper atmosphere with a differential spectrum of the form (4) with exponent 2·9. The deviation from this form at sea level for low energies is due to the decay of the slower rays, and allows an estimate of the mean life to be deduced (Rossi, 1939).

It is not yet clear what is the lower limit of the validity of the law $g(E) \propto E^{-2\cdot9}$ for the mesons at their birth-place. Some measurements by Dymond (1939) at high altitudes suggest that the lower limit may be about 8×10^8 ev. Taking this value as the lower limit, and, from § 2.1, the upper limit as at least 3×10^{11} ev., it is seen that the differential spectrum is roughly of the form $E^{-2\cdot9}$ over a range of energy of at least 400 to 1, and so a range of differential intensity of at least 10^7 to 1.

§ 2.4. *The Soft Component Secondary to the Mesons*

Mesons produce showers in the atmosphere by three processes (§ 1). Since, in addition, large showers are produced by incident primary electrons, there are rather similar showers produced in four different ways. The different processes are therefore very difficult to disentangle.

However, at sea level, below an appreciable thickness of absorber (e.g. 20 cm. Pb), the second and fourth types of shower are no longer present, leaving only those produced by the elastic collision of mesons with electrons and by radiation loss.

If the probability of occurrence of the elastic collisions, as a function of initial energy of the mesons and energy of projected electrons, were known, the observed number of showers under thick absorbers would give evidence for the energy spectrum of the mesons Since, however, this function is not known at high energies, this procedure cannot be employed. In fact the comparison of the observed number of showers with the energy spectrum of the mesons, assuming this to be known, should allow the collision function to be determined. This is a matter of the greatest importance since this function depends on the spin of the meson and on the type of fundamental quantum theory assumed.*

The observed distribution $p(N)$ of showers with more than N rays has long been known to be of the form (Montgomery and Montgomery, 1935)

$$p(N) \propto N^{-s} \qquad \ldots\ldots(5)$$

when $s \simeq 3$, but varies somewhat with the absorber used.

The view has been expressed lately by Oppenheimer (1939) that these observations can be reconciled with the observed energy spectrum of the mesons, by assuming that up to an energy of about 10^{10} ev., the chance $P(E_0, E_1)$ that a meson of energy E_0 will produce a secondary electron of energy E_1 is given by classical theory, that is, $P(E_0, E_1) \propto E_1^{-1}$, whereas for greater energies $P(E_0, E_1)$ is constant.

* For discussion of this problem see Bhabha, Carmichael and Chou (1937); Montgomery and Montgomery (1939); and Oppenheimer (1939).

§ 2.5. *The Protons*

In addition to the mesons, there is found at sea level a proton component, exclusively positively charged, and amounting in number to at least 5 per cent, and possibly 10 per cent, of the mesons. Almost nothing is known directly of the spectrum of these protons, but something may be deduced from consideration of "the positive excess" at sea level. The measurements of the energy spectra show that the number of positive penetrating rays is greater by some 10 per cent than the number of negative rays (Blackett, 1937a; Jones, 1939). There are two possible explanations of this. The whole of the positive excess may be due to protons, with the mesons equally positive and negative ; or it may be due partly to the protons and partly to a real positive excess of mesons. This is an important point which needs investigating. It must be remembered that it is only possible to distinguish a proton from a meson track by its ionization when the energy is comparatively small.

Since the "positive excess" is apparently roughly the same from 10^9 to 2×10^{10} ev., the spectrum of the positive rays must be roughly the same as that of the mesons. If the positive excess is due to protons, this suggests a close similarity between the proton and meson spectrum, and thus an intimate relationship between them. However, since the shape of the meson spectrum at sea level is strongly influenced by the decay of the meson, one would not expect the protons, which do not decay, to have the same spectra at sea level. This holds equally if the protons are of primary origin or if they are produced along with the mesons in the upper levels of the atmosphere. Thus the probable similarity of the spectra of the protons and mesons at sea level may plausibly be interpreted as due to some special circumstance, such as that the protons are produced as secondaries by the mesons.

If, on the other hand, there is a real positive excess of mesons, which has yet to be proved, its effect may completely obscure that of the protons at high energies, thus leaving it impossible to say anything about the spectrum of the protons.

§ 3. SPECTRUM OF PRIMARY ELECTRONS

§ 3.1. *From the Latitude Effect and the Transition Curve*

The variation with latitude of the intensity of the soft component in the atmosphere gives information as to the energy spectrum of the incident rays. By calculating the total energy received at different latitudes from the observed curves showing ionization against height, Johnson (1938), using the results of the Størmer-Lemaître-Vallarta theory of the deflection of charged particles by the magnetic field of the earth, showed that the incident spectrum could be represented by (3), where $n=1{\cdot}87$, that is, almost the same value as deduced from the meson spectrum at sea level. The range of energy over which Johnson showed this law to hold was from 4×10^9 to $1{\cdot}5\times10^{10}$ ev.

This is not by itself a very wide range of energy, but a more detailed analysis of the form of the transition curve in the atmosphere by Heitler (1937) and by Nordheim (1938) and others has made probable an incident spectrum of the

form (3) with $n=1{\cdot}8$ for $E=2\times10^9$ ev., but falling to $1{\cdot}3$ for $E=10^{12}$ ev. However, the analysis cannot be considered as reliable enough to make certain the fall in the value of the exponent.

§ 3.2. *From the Extended Showers*

To get evidence for the spectrum at higher energies, one must go to the remarkable phenomenon of extensive showers discovered by Auger and his co-workers (1939).

These researches demonstrated the existence at sea level of showers with as many as a million rays, spread over an area of over 10,000 square metres. These showers are interpreted as consisting mainly, but not entirely, of electrons and photons produced by a single electron of very high energy incident on the earth's atmosphere. When such an electron arrives it may produce a cascade shower reaching right to sea level. The main flow of energy will lie in a fairly narrow central column containing all the secondary rays of high energy. Surrounding the core will be a much larger column containing the secondaries of lower energy (Euler and Wergeland, 1940). It is also believed that such showers contain an appreciable number of penetrating rays, probably mesons.

Now it is easily deduced from the cascade theory that to produce a shower of 10^6 rays at sea level, an incident energy of 10^{15} ev. is necessary.

The number of these rays of energy greater than 10^{15} ev. has been estimated by Auger *et al.* (1939) from the observed numbers of showers, and is found to be about 10^{-9} ray per sq. cm. per minute. Comparing this number with the known number of rays of energy $1{\cdot}5\times10^{10}$ ev., which is $1{\cdot}9$ per sq. cm. per minute (Johnson, 1938), and assuming the spectrum to be of the form of (3), we find that $n=1{\cdot}92$.

This calculation cannot be at all exact, but the large range of the energies considered makes the value of n only slowly dependent on the exact values assumed for the frequency and energy of the rays producing the large showers.

The above calculation is concerned with the number of rays of 10^{15} ev. and over. We may safely assume that the spectrum extends with the same form to energies well above 10^{15} ev., say to 10^{16} ev. at least.*

We thus have the remarkable result that the spectrum of the incident electrons seems to be of the form E^{-n} with $n=1{\cdot}9$ from an energy of 2×10^9 ev. to at least 10^{15} ev., a range of energy of over $10^6:1$! The corresponding range of the intensity of the differential energy spectrum (4 *a*) is over $10^{18}:1$.

§ 4. GENERAL CONCLUSION AS TO INITIAL ENERGY SPECTRA

We have seen that the experimental evidence suggests that both the differential spectrum of the electrons incident on the atmosphere and that of the mesons when born in the upper parts of the atmosphere are of the form

$$g(E)dE \propto E^{-2{\cdot}9}. \qquad \ldots\ldots(4\,a)$$

* It is of course possible that the spectrum may not be of such a simple form over the *whole* range, but may have some more complicated form over certain energy regions, leaving the form (3) to represent the *average* shape.

For mesons the same form has been demonstrated from 8×10^8 to 3×10^{11} ev. * and for electrons with much less accuracy from 2×10^9 at least to 10^{15} and probably to 10^{16} ev.

Thus at least over the range from 2×10^9 ev. to 3×10^{11} ev. the spectra of the incident electrons and of the newly born mesons are the same.

The interesting question then arises as to whether the meson spectrum extends to as great energy as the electron spectrum. It is only possible at present to guess at this, but there seems no special reason to assume that it does not do so. This question is, of course, intimately bound up with that of the origin of the two types of rays and of their mutual relationship.

§ 4.1. *An Upper Limit to the Electron Spectrum?*

The question now arises as to how far the spectrum extends. Let us suppose that the true energy spectrum is, in fact, of the form E^{-n}, with constant n for values of E up to some critical energy E_c, but that for greater values the form of spectrum is different, for instance falls roughly to zero. This behaviour implies, of course, the existence of a physical quantity E_c, of which we know that it has the dimensions of an energy and is numerically not less than 10^{16} ev.

Now the largest simple atomic constant with the dimensions of energy is Mc^2, which is of the order of 10^9 ev.† Thus E_c is not likely to be an atomic constant, but might rather have some cosmological origin. Failing any theory of the origin of the rays, and knowing that E_c is greater than 10^{16} ev., we have no reason to suppose that it is not much greater than 10^{16} ev. In other words, we have at present no reason to suppose that the power-law spectrum does not extend to energies much greater than 10^{16} ev.

§ 4.2. *Some Properties of Rays of very great Energy*

The theory of the ionization by particles of great energy as developed by Williams (1933) shows that the breadth of the track of ionization produced by a particle increases in proportion to the energy. This arises from the Fitzgerald contraction of the electric field of the particle, due to its very high velocity ; this contraction of the field increases the sharpness of the pulse falling on the atoms and so causes ionization at much greater distances than for slower particles. Williams shows that the breadth of an electron track is proportional to its energy and is about 0·05 cm. for $E = 10^{10}$ ev. Thus an electron of 10^{15} ev. will produce a track 50 m. broad, and one of 10^{16} ev. a track 500 m. broad !

This fact will have to be taken into account in calculating the breadth of the distribution of ionization in cascade showers high up in the atmosphere, where the average energy of the shower particles is very high.

* One must draw attention again to the possibility that the exponent increases at the greater energies (§ 2.1).

† Of course Mc^2 might be multiplied by any of the non-dimensional ratios, e.g. by $(M/m)^2 \simeq 4 \times 10^4$, to give higher energies. But this seems a somewhat artificial procedure.

If the meson spectrum extends as far as the electronic spectrum, some of the mesons will have large penetrating powers. In fact it can easily be calculated, assuming an energy loss 2×10^6 ev./cm. H_2O, that, provided no new form of absorption enters at high energies, a meson of energy 3×10^{16} ev. should be able to traverse the whole diameter of the earth!

The interesting possibility arises that it may prove possible to detect single cascade showers by the reflexion from them of radio waves. Short-lived radio reflexions are of course well known. It has been shown recently (Blackett and Lovell, 1941) that very large cascade showers initiated by electrons of energy 10^{16} to 10^{17} ev. should produce enough ionization to give a detectable reflexion of a suitable radio signal. If this is verified in practice, a way is opened to extend the investigation of the upper end of the electronic spectrum to still higher limits.

§ 5. SOME SPECULATIONS ON THE ORIGIN OF THE MESON

§ 5.1. *The Photon-origin Hypothesis*

The most plausible assumption is that the mesons (Y^+, Y^-) are formed in the upper atmosphere in the process of the absorption of energetic secondary photons ($h\nu$) by the protons (P) and neutrons (N) in nuclei, according to the scheme

$$\left.\begin{aligned} h\nu + P &\rightarrow N + Y^+, \\ h\nu + N &\rightarrow P + Y^-. \end{aligned}\right\} \qquad \ldots\ldots(A)$$

In this hypothesis there is, however, the serious difficulty that these processes are reversible, for the mesons created by this process would collide with protons and neutrons and emit photons, and the cross-section for the two processes in the opposite direction would be about the same. Experiments show, however, that the reverse process does not occur with sufficient frequency. Hence process (A) is unlikely to be the origin of the mesons.

In general one can reason thus:—The mesons are produced mainly in the upper level of the atmosphere and, therefore, by some primary radiation which is highly absorbable. Since, however, the mesons themselves are very penetrating, the process by which they are born cannot be a reversible one.

One is led therefore to consider the following obviously irreversible multiple processes *:—

$$\left.\begin{aligned} h\nu + P &\rightarrow N + Y^+ + Y^- + Y^+ + \ldots \\ h\nu + N &\rightarrow P + Y^- + Y^+ + Y^- + \ldots \end{aligned}\right\} \qquad \ldots\ldots(B)$$

Heisenberg (1939) had predicted the existence of such multiple showers from theoretical considerations, but later formulations of the theory by Bhabha do not lead to such multiplication processes (Bhabha and Corben, 1941).

Another possible process is that of the production, say, of meson pairs by the interaction of photon or electron with the Coulomb field of a nucleus. This is analogous to the production of electron pairs.

* See general discussion by Heitler (1939) and Nordheim (1938).

§ 5.2. *The Proton-origin Hypothesis*

There is one argument which, if valid, excludes the process (B) from being the origin of the mesons. Johnson (1939) has deduced from the latitude effect and azimuthal asymmetry of the penetrating component that the primary rays producing the mesons are exclusively positively charged, while the primary electronic rays are about equally positive and negative. The interpretation of the data is essentially complicated, as it depends on the refinements of the Størmer theory and on the not yet completely known process of absorption in the atmosphere. If, however, Johnson's conclusion be provisionally accepted, as it seems it should be on the present available evidence, one must look for a stable positively charged ray as the parent of the meson.

Johnson assumes that these rays are protons, and that they create mesons by collision with protons and neutrons. This can happen in the following ways *:—

$$\left.\begin{aligned} &P+P\rightarrow P+N+Y^{+},\\ &P+N\rightarrow P+P+Y^{-},\\ &P+N\rightarrow N+N+Y^{+}. \end{aligned}\right\} \qquad \ldots\ldots(C)$$

These processes are effectively irreversible, and so can be accepted as possible processes.† Multiple processes such as

$$P+P\rightarrow P+N+Y^{+}+Y^{-}+\ldots \text{ etc.} \qquad \ldots\ldots(C')$$

are also possible, but it is not necessary to assume them to explain the penetration of the mesons.

If processes (C) occur, an incident proton must be rapidly absorbed in the atmosphere, for instance in an absorption distance of the order of a metre of water. It follows that the observed protons at sea level cannot be of primary origin, since they would never reach sea level, but must be secondary to the mesons, being produced, presumably, by elastic collision. This result fits in with the conclusion of § 2.5 that the protons must be intimately related to the mesons.

§ 5.3. *Comparison of the Two Hypotheses*

On the simplified form of photon-origin hypothesis, the only primary rays will be equal numbers of positive and negative electrons, with the energy spectrum (4 *a*). These produce mesons with the same spectrum in the upper atmosphere by the process (B), together with some protons. The place of the protons in this scheme is not clear. To them, however, must be attributed in some way the whole of the azimuthal asymmetry of the penetrating component, though how they can do this if they are secondary to a radiation which is equally positive and negative is not easy to see.

A modification of the hypothesis is to assume that there are primary protons

* For a possible observed case of these processes, see Wilson (1939).

† One notices two ways of producing positive mesons and one way of producing negative mesons. One wonders whether this may have some relation with the hypothetical positive excess of mesons (§ 2.5).

which penetrate the whole atmosphere and themselves form the observed protons at sea level.

On the proton-origin hypothesis, the incident primary rays consist of equal numbers of positive and negative electrons together with a positive proton component. Both must be assumed to have the same spectra. The protons then produce mesons, either equally positive and negative, or with a small positive excess, with the same spectrum as themselves. The sea-level protons are then secondary to the mesons.

If this is the correct picture of the origin of the mesons, then one can see a possible reason why the two primary components, the electrons and the protons, should have the same spectra. For assume that protons are produced somewhere in the universe and that they then pass through a considerable mass of localized matter. Then, according to our assumption, the protons will produce mesons, with the same spectral distribution as the protons producing them. The mesons will again by elastic collision produce more protons of the same spectral distribution. But on leaving the mass of matter, the mesons will decay into electrons (and positrons) with the same spectral distribution as the mesons, and so the same as that of the protons. Thus what will arrive at the earth is a mixture of protons and electrons with the same spectral distribution. On this interpretation the primary soft component is mainly the decay spectrum of mesons produced by protons.

Whether this is the correct picture or not, it is certainly true that if the stream of cosmic-ray particles, as observed, say, at sea level, was let out again directly into space, instead of being further absorbed, then the mesons would rapidly decay, leaving a mixture of protons and electrons.

§ 5.4. *Speculation on the Origin of the Inverse-1·9-power Spectrum*

This wide range of validity of the power-law form $g(E) \propto E^{-2\cdot9}$ for the differential energy spectrum entitles it to be taken seriously as a rough law of nature.

A spectral distribution which has such a simple form* over a range of energy of over $10^6 : 1$, and of a differential intensity of over $10^{18} : 1$, can hardly have an origin in some "accidental" property of stellar systems. Rather one inclines to the view that this law must have some relatively fundamental origin.

For instance, one may think of it perhaps as having some physico-geometrical origin like the other main inverse-square laws of nature, propagation of energy, variation of electric, magnetic and gravitational energy with distance.

A second possibility is that the observed spectrum is the result of the dynamics of some type of collision. Just as the spectrum of secondary electrons produced by the classical collision of a very fast particle with free electrons is of the form dE/E^2, so the typical cosmic-ray spectrum $dE/E^{2\cdot9}$ may be due to the collision

* Actually the experimental data are only accurate enough to determine n roughly, e.g. with an error of, say, $\pm 0\cdot1$. So, for instance, the exponent may well be 2·0 and not 1·9.

with electrons or protons of some other particles of energy greater than the upper limit (if any) of the spectrum.

A third possibility is that the law is of statistical origin, being comparable in type to Maxwell's law of distribution of velocities ; in other words, it may be due to the result of some kind of fluctuation phenomena in cosmic space.

A fourth possibility is that it may be related to the structure of the universe as a whole, and so should be placed in the same class as the law of the apparent recession of the nebulae. One probable relation between cosmic-ray phenomena and cosmology has already been pointed out (Blackett, 1939). By dimensional arguments it was shown that the mean life of a meson at rest probably depended on the gravitational constant, and so, through general relativity theory, on the total mass of the universe.

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THE HEAT CONDUCTIVITY OF RUBBER AT LOW TEMPERATURES

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ABSTRACT. The heat conductivity of rubber has been measured in the range between liquid air and room temperatures, employing a mechanically operated cryostat which is fully described. The heat conductivity shows an abrupt rise between 190° K. and 220° K.

§1. INTRODUCTION

About a year ago I reported the results of some qualitative measurements of the heat conductivity of rubber at liquid-air temperature (Schallamach, 1940). The values at the low temperature turned out to be considerably smaller than at room temperature. I then wished to find out whether the heat conductivity decreased continuously from room to liquid-air temperature or whether this drop was in any way connected with anomalies of other physical quantities occurring in the neighbourhood of 200° K. At this temperature, for example, the specific-heat curve shows a sudden drop (Ruhemann and Simon, 1928; Bekkedahl and Matheson, 1935), and the specific-volume curve has a well-defined kink (Bekkedahl, 1934).

For this reason, heat-conductivity measurements were carried out to cover the intermediate region between liquid-air and room temperatures, the results of which experiments are the substance of this paper.

The cryostat used in this investigation is thought to be of a somewhat novel type and, at the same time, of rather simpler design than low-temperature thermostats previously described in the literature. As it may find application in other spheres of low-temperature research, it is described in detail. The actual heat-conductivity apparatus is a modification of Eucken's (Eucken, 1911).

§2. THE CRYOSTAT

The principle underlying this thermostat is to make and break, by manipulation outside the apparatus, a mechanical heat contact between the cooling medium and the component or part which is to be maintained at an intermediate temperature. A cross-section of the arrangement is shown in figure 1.

The vacuum enclosure 1 houses the device and is evacuated through tube 2. The liquid-air container 3, suspended from the lid of vessel 1 by two tubes, is filled through one of them, the spout 4. The part marked 5 holds the con-

ductivity apparatus inside the radiation screen 6, and it is this component which is to be kept at intermediate temperatures. Parts 5 and 3 are lined up with respect to each other and, at the same time, thermally insulated from each other by the short Perspex tube 7. Perspex is a poor heat conductor which stands up well to low temperatures and has been found most satisfactory.

Ignoring for the moment pin 12, the heat contact between the liquid air in 3 and the apparatus holder 5 is made through the screw 8. This screw is worked

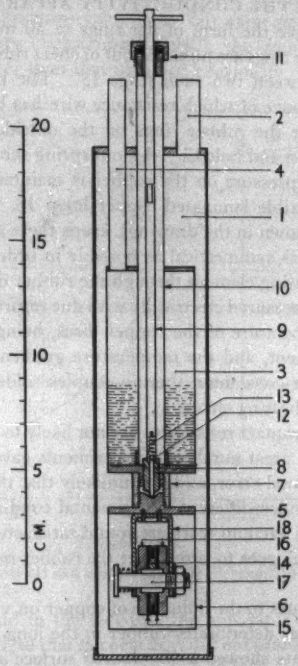

Figure 1. Cross-section of apparatus.

by means of the stem 9, which is brought out through tube 10 and the packing gland 11 on top of it. This screw has a conical bottom fitting into a corresponding seating in 5. This gives a certain amount of self-centering. When the screw is turned down as far as it will go, the heat contact is established and the specimen cools down. As soon as the desired temperature is reached, the screw is loosened and only tightened again at intervals to compensate for any rise in temperature due to radiation or heat generation inside the apparatus attached to 5.

A smoother working is achieved with the help of the pin 12, which in effect is a variable heat resistance. Spring 13 presses the pin against the seating in 5, and this pressure and the free length of the pin protruding from the screw determine the conductance of the contact. By judicious initial adjustment the temperature can be kept constant without interference for a considerable time.

It should not be difficult to make the device self-adjusting by appropriate electro-mechanical means.

§3. THE CONDUCTIVITY APPARATUS

The specimens have the form of flat rings of 30 mm. outside and 10 mm. inside diameter. The rings are punched out of sheet rubber. A heating element 14 is sandwiched between two such rings 15. The heating element is a flat copper reel, in the groove of which resistance wire has been wound. Two solid copper discs 16 cover the rubber rings on the outside, the whole being held together by the 1/4" nut and bolt 17. A stout spring shown on the left side of the bolt ensures that the pressure on the rubber is maintained in spite of thermal contraction. The flexible laminated copper hoop 18, which is screwed to the discs 16 (screws not shown in the drawing), keeps these discs at the low temperature. The design is as symmetrical as possible in order to be sure of an equal heat flow from the heating element through the rubber discs.

The heat input is measured electrically with due regard to losses in instruments and leads. The temperature of the copper discs, being the reference temperature of the measurement, and the temperature gradient across the rubber, are measured with copper-constantan thermo-couples soldered flush into the faces of the disc and of the heating element.

We think that the contact resistances are not likely to falsify the results for the following reasons. A great number of experiments gave the same result within the limits of experimental error, and it is unlikely that the contact resistance was exactly reproduced under different experimental conditions. Apart from that, rubber gaskets used in vacuum work are found satisfactory even at low temperatures, which fact rather goes to prove that the rubber-metal contact is very good indeed.

Another doubt relates to the influence of copper on vulcanized rubber. It is well known that copper deteriorates rubber in the long run. The rubber discs used in our experiments showed a discoloured surface after prolonged insertion in the apparatus, but otherwise there was no obvious change in the elastic properties, nor did fresh discs give different results from old ones.

§4. RESULTS

The heat-conductivity measurements were carried out with " North British " cycle tube which is labelled as pure latex, electrodeposited; thus there are no fillers to be expected, apart from sulphur and accelerators. The results are shown in figure 2, where the measurements are given as short horizontal lines the lengths of which are equal to the actual temperature differences found across the rubber

discs. Accordingly, the heat conductivity given by the ordinate of these lines is an average value in that range. The lines connected by the full thick curve I represent measurements made after having first cooled down the specimens to about 100° K. The reason for this procedure will be pointed out presently.

The heat conductivity rises steadily up to about 190° K. Near this temperature, the curve begins to rise more quickly, the maximum gradient being reached in the vicinity of 210° K. At 220° K. this sharp rise gives way again to a slow increase up to room temperature.

Whereas the low-temperature value is in fair agreement with our findings in the earlier experiments (Schallamach, 1940), the room-temperature value turns out to be much lower than that obtained previously. The reason for this

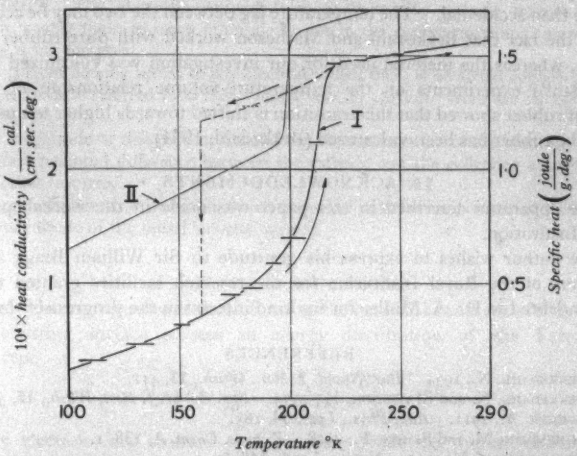

Figure 2. Thermal conductivity (I) and specific heat (II) (Bekkedahl and Matheson) of rubber.

discrepancy is to be found in the fact that the apparatus used before was not symmetrically constructed, so that the heat flow did not branch off equally through both discs.

If the measurements begin at room temperature or any temperature above the second turning point of the curve I, the result is rather different. The heat conductivity declines comparatively little at first until it suddenly drops to a value on the ascending curve I, generally well below the first turning point of this curve. This behaviour is indicated by the broken descending curve which, in contradistinction to the ascending curve, depends very much on the experimental conditions, mainly on the speed of cooling. Sometimes the temperature could be lowered as far as 100° K. before the drop occurred. This critical point is easily noticed

without any measurements at all because it is accompanied by a clicking noise which is probably familiar to anybody who has ever handled rubber in liquid air.

§ 5. CONCLUSION

The experimental evidence points to the fact that a transition of some kind takes place at about 210° K., which, however, in consequence of supercooling, may take effect at a considerably lower temperature.

As is mentioned in the Introduction, there is other evidence of such a transition. The thin curve II in figure 2, which is taken from the paper of Bekkedahl and Matheson (1935), gives the specific heat of amorphous rubber. A sudden rise of this quantity occurs just below 200° K., and we feel sure that this near coincidence is more than accidental. The temperature lag between the two may be accounted for by the fact that Bekkedahl and Matheson worked with pure rubber hydrocarbon, whereas the material used for our investigation was vulcanized rubber. Bekkedahl's experiments on the temperature-volume relationship of various kinds of rubber showed that this transition is shifted towards higher temperatures when the rubber has been vulcanized (Bekkedahl, 1934).

§ 6. ACKNOWLEDGEMENTS

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The author wishes to express his gratitude to Sir William Bragg and the Managers of the Royal Institution for the research facilities granted to him. He is indebted to Dr. A. Müller for his kind interest in the progress of this work.

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INITIAL VELOCITY CURRENTS IN THERMIONIC VALVES

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ABSTRACT. The electrons emitted from a thermionic cathode possess enough energy to enable them to reach other electrodes in the system providing the opposing potential is not too great.

The current flowing across to any electrode when there is no actual applied potential on the electrode is referred to as the "initial velocity current" from the cathode to that electrode.

The magnitude of this current depends on the temperature of the emitting cathode, the contact potential difference between the cathode and the collecting electrode, and several other features.

Experimental results are given which show the relative importance of these effects on the magnitude of the initial velocity current.

§1. INTRODUCTION

The electrons inside the metal forming the base of the cathode of a thermionic emitting surface possess an energy distribution of the Fermi-Dirac type:—

$$N_E\,dE = \frac{8\pi}{h^3}\cdot\frac{\sqrt{(2m^3E)}}{(e^{-(\mu-E)/kT}+1)}\cdot dE.$$

The electrons are emitted thermionically from the upper surface of the Fermi-Dirac distribution and possess an energy distribution in accordance with Maxwell's formula:

$$\frac{4\pi n_0\sqrt{2E}}{(2\pi kT)^{3/2}}\,e^{-E/kT}\cdot dE.$$

This means that a considerable number of electrons possess an energy sufficiently great to carry them over an opposing potential barrier of one or two volts.

If, in any given space of time, N_0 electrons have been emitted, then the fraction of this number which possess energy sufficient to overcome a reverse potential of value V_r volts is given by

$$\frac{N}{N_0} = e^{\frac{-11600V_r}{T}}.$$

It is not easy to determine N by measuring the ratio of the initial velocity currents at $V=V_r$ and $V=0$, because the current ratio is very different from the

ratio of number of electrons emitted. This is due to the effect of space-charge potentials which are set up between the emitting surface and the collector.

The work function of the cathode which is emitting electrons is usually less than that of any other electrode in the valve. Thus the work function of the typical barium-strontium oxide cathode is 0·9 volt, whereas the work function of clean molybdenum is 4·17 volts and that of clean nickel is 5·02 volts. The contact potential existing in the space between two materials is given by V, where

$$V = \phi_A - \phi_B - P.$$

In this equation P is the Peltier e.m.f. and ϕ_A and ϕ_B are the respective work functions of the two materials. The Peltier e.m.f. P is of the order $10^{-4}T$ and is negligible at ordinary temperatures compared with ϕ_A and ϕ_B. Thus to a good approximation we can say that the contact potential difference is equal to the difference between the work functions of the two materials.

The contact potential difference between an oxide cathode and a nickel and molybdenum sheet will be

Oxide cathode—nickel	4·12 volts,
Oxide cathode—molybdenum	3·27 ,,

and the potential is in such a direction that it will oppose the flow of electrons from the cathode to the other electrode.

Some electrons, by virtue of their initial velocity, will have enough energy to overcome this opposing voltage and will give a small electron current flowing from cathode to grid or other neighbouring electrode. This is usually referred to as the " initial velocity current " from the cathode, and this current, which we will denote by i_{ec}, plays a significant part in the operation of the valve.

In the case of the diode detector, for example, the initial velocity current in passing from the cathode through the diode load, determines the steady average potential of the detector anode before the signal is received and also affects the curvature of the detector response at low input voltages.

The associated factor, namely, variation of the work function, affects the value of the anode current, especially when the anode current is not very large having regard to the mutual conductance (high amplification factor and high impedance). It appeared to be desirable to carry out some fundamental investigations regarding the behaviour of these initial velocity currents and the way in which their magnitude may be controlled. These experiments are described in this paper and some general deductions are made from the results.

The paper is divided into the following sections :—

(1) Order of magnitude of the initial velocity current for different types of valves.
(2) Relationship between the initial velocity current and the retarding potential necessary to reduce the current to zero.
(3) Dependence of initial velocity current upon gas softness in the valve.
(4) Effect of temperature on the initial velocity current.

(5) Effect of variation of inter-electrode spacing on the magnitude of the initial velocity current.
(6) How initial velocity current may be controlled by suitable processing.
(7) Variation of initial velocity current during the life of the valve.

The experiments were carried out using diode valves, also triodes, pentodes and other general types. The collecting electrode was one or other of the grids or, on some occasions, the anode itself. The initial velocity current will be spoken of as a positive current if it flows through the micro-ammeter in the grid circuit (or circuit of the collecting electrode) in such a direction that it represents electrons arriving at that electrode, and as a negative current if it flows in such a direction that it represents electrons leaving that electrode.

§2. NATURE AND ORDER OF MAGNITUDE OF THE INITIAL VELOCITY CURRENT

The first series of observations was taken on a typical indirectly heated mains triode of medium impedance and specified as follows:—

Heater volts = 4·0.	Impedance = 12·500.
Heater current = 1·0 amp.	Amplification factor = 40.
Anode volts = 200.	Mutual conductance = 3·3 ma./v.

The initial velocity current flowing from cathode to grid of such a valve when there is no applied voltage on the anode, and when the cathode is at a normal working temperature, is 530 micro-amperes through a galvanometer of resistance 900 ohms. Similarly, the initial velocity current to the grid of an output pentode may be of the order of 600 micro-amperes.

In table 1 we give the values of the initial velocity current from cathode to inner grid for a number of different types of mains valves, along with some of the more relevant features of their design.

Table 1

Type of valve	Heater volts	Heater current amps.	Cathode section	Grid 1 wire molyb-denum	Grid 1 t.p.i.	i_{ve} with 1 megohm in series	i_{ve} with no res. in series
VPT4	4	1	·041″ × ·079″	·004″	35	·085 μa.	·430 ma.
VHT4	4	1	·041″ × ·079″	·004″	25	·075 μa.	·180 ma.
D4	4	1	·041″ × ·079″	·004″	50	·1·00 μa.	·530 ma.
PT4D	4	2	·140″ × ·040″	·003″	52	·08 μa.	·270 ma.
LP4	4	1	Fil ·0042″	·006″	16	·025 μa.	·017 ma.

The effect of design on the magnitude of this initial velocity current will be dealt with in a later section. Thus it will be seen that, in general, some of the electrons from the filament or cathode possess sufficient energy to enable them to overcome the opposing negative potential barrier presented by the grid. This is, however, not always true, and details will now be given relating to a mains triode of type D4 in which the opposing negative potential of the grid was sufficient to prevent all the electrons from getting across at normal working temperature.

Negative initial velocity current on mains triode type D4

A valve of standard design D4, in accordance with the details given in table 1, was found to give a negative initial velocity current and was tested under the following initial conditions:

anode voltage.....$v_a = 270$,
filament voltage.....$v_f = 4{\cdot}0$,
grid voltage.....$v_g = 0$.

The anode current was then 10 ma.

The initial velocity current (which we will hereafter denote by i_{vc}) was $-1{\cdot}3$ micro-amperes through a protective resistance of 1 megohm in series with the galvanometer. This current could not be due to electrons reaching the grid, but it could be due to electrons leaving the grid (grid emission) or to positive ions drifting towards the grid because of its appreciable negative potential relative to the cathode. This was easily decided by disconnecting the lead to the cathode circuit, but leaving the cathode at normal temperature in order to keep the grid also at its working temperature. The grid was still connected to H.T. negative through the 1-megohm resistance and the micro-ammeter as shown at point Y,

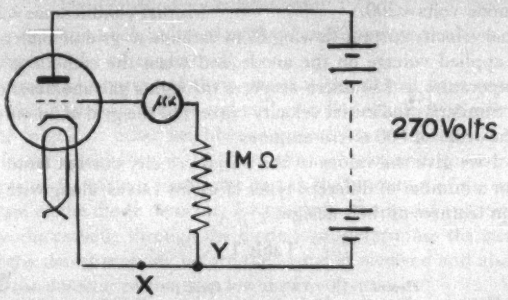

Figure 1.

figure 1. The circuit to the cathode was broken at the point X only. Under these conditions there was then no current through the micro-ammeter in the grid circuit.

The grid current would have persisted if it had been due to grid emission (to anode); also if it had been due to inter-electrode electrical leakage between grid and anode.

Keeping the anode voltage at 270, the grid was disconnected and allowed to float (instead of being connected to cathode through the 1-megohm resistance in series with a micro-ammeter). The anode current then increased from 10 ma. to 11·35 ma. Also this valve gave an indication of softness current of 2 micro-amperes when the softness was measured in the usual manner, by taking the

grid slowly negative through a micro-ammeter and a 1-megohm protective resistance, until the peak grid-current was recorded. The increase in the anode current when the grid is allowed to float can be accounted for by assuming that the natural negative contact-potential of the grid relative to the cathode is so high that no initial velocity electrons can overcome this barrier. When the grid is disconnected, the positive ions will be attracted to it as before until they have so increased its positive charge that it no longer repels the negative electrons from the filament, and so the grid remains at a potential, such that the number of positive ions reaching the grid just neutralizes the amount of charge carried by the initial velocity electrons from the filament. The final result is that the grid becomes slightly less negative when floating than when earthed to cathode, and so the anode current increases.

If the valve had been perfectly gas-free the anode current should not have increased. This is never attained in practice, however, because even if the gas content were so low that no actual positive-ion current were observed, the grid, having a natural high negative contact-potential, would attract sufficient positive ions to reduce this potential as soon as it becomes floating.

Although the energy of the electrons was not sufficient to enable them to climb over the potential barrier at normal working temperature, nevertheless they will probably be able to get across at a slightly higher temperature. The next experiment therefore was to measure the current to the grid for various values of filament temperature, with and without the anode potential applied to the valve. The results of this experiment are given in table 2.

Table 2

Heater volts	Heater current	Anode at 270 volts		Anode at zero	
		i_{vc} (μa.)	i_a (ma.)	i_{vc} (μa.)	i_a (ma.)
1	0·6	− ·15	·05	0	
2	0·82	− ·25	·2	0	
3	1·05	−1·3	2·6	0	Zero
4	1·17	−1·3	11·7	0	in each
5	1·31	−1·0	22·0	+ ·2	case
6	1·45	− ·5	29·0	+ ·3	
7	1·60	− ·4	45·0	+ ·5	
8	1·70	− ·3	72·0	+ ·8	
9	1·85	+ ·1	106·0	+1·2	

Thus as the heater volts increase, the initial velocity current to the grid (if $v_a=0$) remains at zero (column 5) until the cathode heater voltage reaches 5. At this point the energy of a sufficient number of electrons at the upper limit of the Maxwell distribution is sufficiently large to enable these electrons to pass across to the grid and so give an i_{vc} value in the direction usually observed from

this type of valve. If the anode potential is not zero, however, then the i_{vc} relation to cathode voltage takes up a very different form (see table 2, column 3). Thus in this case there is a negative i_{vc} even at $v_f = 1{\cdot}0$ volt, and this increases to a maximum value of $-1{\cdot}3$ micro-amperes at $v_f = 4$ volts. For increasing values of v_f, i_{vc} decreases until it crosses the zero axis between $v_f = 8$ and $v_f = 9$ volts, and then actually becomes positive in a similar manner to the normal i_{vc} current as listed in table 1. All these results can clearly be explained in terms of positive-ion current to the grid. Thus when the anode voltage is applied there will be some emission drawn from the cathode even at $v_f = 1{\cdot}0$ volts, and this will produce some positive ions by collision with the residual gas present in the valve. These positive ions then give the negative i_{vc} of $-0{\cdot}15$ micro-amperes. Also, as the cathode temperature is increased, the emission will be increased and the positive-ion current will also increase (as is seen by $-1{\cdot}3$ μa. at $v_f = 4$ volts).

However, about this point the initial velocity of a number of the electrons is great enough to enable them to overcome the negative barrier set up by the grid, and so these electrons begin to reach the grid and reduce the positive-ion current. This explains why the positive-ion current has its maximum of $-1{\cdot}3$ μa. at 4 volts (as seen from column 3), because just after this (at $v_f = 5{\cdot}0$ volts) the electrons are able to reach the grid as seen from the figures in column 5. Finally, at 9 volts the electrons have so much energy that they are able to provide a grid current which overcomes the positive-ion current and so we have a value of $+0{\cdot}1$ micro-amperes for i_{vc} at $v_f = 9{\cdot}0$ volts ($v_a = 270$ volts).

The above experiments lead to the following conclusions :—

(1) The grid contact-potential is in such a direction that it opposes the free flow of electrons from the cathode when the latter is at normal working temperature.

(2) The grid may be sufficiently highly negative to prevent any electrons from the cathode reaching it at normal cathode temperatures. This is rarely the case with valves having indirectly heated cathodes. This highly negative contact-potential of the grid usually occurs when the valve is showing an appreciable gas content. A trace of gas prevents the grid becoming activated and so prevents the work function of the grid from becoming more nearly like that of the cathode.

(3) The negative contact potential of the grid, although high, is normally not high enough to prevent an appreciable number of electrons from reaching the grid from the cathode. These electrons constitute the normal initial velocity current observed passing from cathode to grid with no externally applied voltage on the grid. The condition usually exists in a valve showing very little or no softness current. It usually means that the grid has become slightly activated by the alkaline earth metals and their oxides which have been evaporated from the filament. This evaporation is more likely to have occurred in the case of a valve which is hard than in the case of a valve which is soft, because the filament of the hard valve will most likely have been raised to a higher temperature than that of the soft valve.

§ 3. RELATIONSHIP BETWEEN THE INITIAL VELOCITY CURRENT AND THE POTENTIAL REQUIRED TO REDUCE THE CURRENT TO ZERO

If we consider a number of identical valve assemblies having the same filament temperatures, the same filament-grid spacing and all other features as closely alike as possible, we should expect the initial starting currents and the retarding potentials (V_r) to be alike. In actual practice, taking normal production valve assemblies, we find that these valves differ considerably from one assembly to another. Also, if we select a number of assemblies for which the starting currents are alike, we find that this does not mean that the retarding voltages will be alike.

These results are illustrated by the figures given in table 3.

Table 3. Initial velocity current and retarding potential for battery 2-volt triode valves

i_{vc} = initial velocity current,
V_r = retarding voltage.
Taken at $v_f = 2{\cdot}0$ volts,
$v_a = 0$.

Valve no.	i_{vc} (μa.)	V_r retarding volts (to reduce i_{vc} to 0·1 μa.)
1	3·6	·60
2	2·3	·50
3	2·0	·46
4	1·3	·40
5	2·3	·41
6	1·6	·45
7	0·7	·40
8	3·2	·58
9	1·6	·41
10	2·3	·58
11	3·0	·52
12	3·7	·50
13	2·6	·40
14	3·1	·57
15	1·6	·39
16	3·3	·49
17	3·1	·50
18	2·7	·42
19	2·7	·50

The figures in table 3 indicate that there is a general statistical connection between the initial starting current and the negative retarding potential required to reduce the current to 0·1 micro-amp. Thus the larger the value of i_{vc} the greater the retarding potential necessary to reduce it to zero, but for any given value of i_{vc} the actual retarding potential V_r may vary over a range of $\pm$ ·05 volts. Difference in filament temperature could account for some of this variation, but the effect is not large enough to explain the actual variations observed in practice.

The other main reason is the varying degree of " patchiness " of the emissive coating, which results in rather different space-charge conditions being set up around the respective filaments. These space charges have a very considerable effect on the magnitude of the initial velocity current. Thus, if the emission from the filament is uniformly distributed, i_{vc} will be higher than that from a filament whose emission comes from patches of the active surface, although the actual retarding potential may be the same in each case.

§ 4. INITIAL VELOCITY CURRENT AND SOFTNESS IN THE VALVE

This problem has been dealt with from another point of view in the first section.

If the valve is soft it is probable that the contact potential of the grid will be sufficiently negative to prevent any of the initial velocity electrons from reaching the grid, but will allow positive ions to travel to the grid if the positive ions are formed in the valve due to the primary electron current.

In this case the valves will show no i_{vc} unless anode current is also drawn across, and then it will give negative values of i_{vc} because the grid will be attracting the positive ions. If the grid is changed from the cathode potential to become floating, or tied to cathode through a high resistance, then the anode current will increase considerably just as we observed in the case of the D4 valve described in the first section. This increase is due to the positive ions reaching the grid and giving the grid a less negative potential. On the other hand, in the case of a normal gas-free valve showing a few micro-amperes of initial velocity current, the anode current will decrease if the grid is left floating or earthed to cathode through a high resistance.

These effects are shown very clearly by the results listed in table 4. These figures were obtained on small battery triodes. The three valves in the first set were given a low activation schedule on the automatic pump and thus were " soft " valves. The other three were given the normal schedule and so were " hard " valves.

Table 4. Effect of softness on floating grid potential.
(Anode current with $v_a = 120$ volts and $v_f = 2$ volts.)

	Anode current in milliamperes ($\pm$ ·005 ma.)		
Valve no.	Grid earthed to neg. end of fil.	Grid earthed through 0·5 megohm	Grid floating
Soft valves			
1	0·59	0·62	0·82
2	0·53	0·55	0·79
3	0·59	0·61	0·82
Hard valves			
4	0·90	0·88	0·86
5	0·89	0·88	0·87
6	0·89	0·87	0·86

It will be seen that when the valves are showing softness current, the anode current increases when the grid is allowed to float, but when the valves are hard the anode current decreases when the grid is floating.

There is an intermediate condition in which the vacuum is sufficiently good to enable the true initial velocity electronic current to neutralize the positive-ion currents exactly, and in that case there will be no actual observed i_{vc}, and no change in anode current when the grid is allowed to float.

§ 5. EFFECTS OF TEMPERATURE ON INITIAL VELOCITY CURRENT

The observations made here on the dependence of the i_{vc} on temperature are not intended as a check on the thermal energy distribution of the emitted electrons, but are practical illustrations as to how these initial velocity currents respond to temperature alterations of the filament.

In table 5 the values of the initial velocity current and the retarding voltages for simple 2-volt battery triodes are given for various values of the filament voltage.

Table 5

Fil. volts.	Fil. current (ma.)	No. 1 valve		No. 2 valve		No. 3 valve		No. 4 valve	
		i_{vc}	V_r	i_{vc}	V_r	i_{vc}	V_r	i_{vc}	V_r
1·5	75	0·4	0·20	0·25	0·25	0·25	0·20	0·2	0·25
2·0	90	3·6	0·60	2·3	0·50	2·0	0·46	1·3	0·40
2·5	107	12·0	0·80	9·3	0·70	10·3	0·80	6·2	0·75
3·0	122	26·0	0·85	27·0	0·78	22·0	0·90	17·5	0·78
3·5	137	44·0	1·10	42·0	0·95	35·0	1·10	28·0	0·85
4·0	151	54·0	1·20	54·0	1·17	47·0	1·20	35·0	0·85

The i_{vc} values were measured by means of a micro-ammeter of resistance 900 ohms, and there was no other resistance in series with the micro-ammeter. It will be observed from the figures given in table 5 that the values of i_{vc} increase together in all cases, and an increase of i_{vc} due to an increase in temperature is accompanied by a corresponding increase of V_r. In this way the results for any one valve assembly never give the discrepancies between the values of i_{vc} and the retarding potential such as we observed in the case of i_{vc} values for separate assemblies, although they were nominally identical in structure. The increase of i_{vc} and the retarding potential could be compared with the values calculated from the Maxwell energy distribution, but this was not intended when these figures were recorded.

Similar results were obtained using valves with indirectly heated cathodes. In these cases the initial velocity current can become several milliamps. at higher cathode heating than normal.

§6. EFFECT OF INTER-ELECTRODE SPACING ON INITIAL VELOCITY CURRENT

The initial velocity current is very much modified by the spacing between the collecting electrodes and the emitting surface. This is due to the retarding effect of the space-charge cloud around the cathode. These effects can be seen from the following values of the initial velocity current as recorded to the various electrodes of a double diode output pentode valve of type PT4D.

Current to grid with no opposing voltage	= 1000 μa.
Current to screen with grid tied to cathode	= 170 ,,
Current to anode with grid and screen tied to cathode	= 73 ,,
Current to screen with grid floating	= 0 ,,

The inner grid to cathode spacing in this type of valve was 0″·015.

It is interesting to compare the above figures with those obtained from a special assembly made up with an identical type of cathode heater system, but with two grids of the same type and wire diameter as that used on the PT4D, but with each grid removed a considerable distance from the cathode surface. One grid (G1) is situated 6 mm. from the cathode (cathode surface to central plane of the grid), and the other grid (G2) is situated 3 mm. on the other side, as shown in figure 2. The grids were made from 0·003 molybdenum wire of 52 t.p.i. on

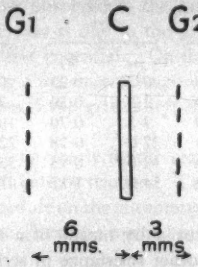

Figure 2.

Kulgrid supports. The cathode was the standard Ba- Sr oxide-coated cathode as used on the normal output pentode.

The figures in table 6 give the values of i_{pc} (in micro-amperes) to G1 and G2 for various values of heater input to the cathode.

Table 6

Heater volts.	Heater amps.	Current to G1 (μa.)	Current to G2 (μa.)
1	1·00	0	0
2	1·42	1	3
3	1·80	5	39
4	2·10	8·3	73
5	2·39	13·0	110

The figures (table 7) indicate the initial velocity currents to G1 and G2 and also to the inner control grid of a standard valve for various values of the applied retarding voltage. The cathode was run at 4 volts, 2·06 amp. on the special assembly and 4 volts, 2·02 amp. on the normal valve.

Table 7

Retarding volts.	Current to G1	Current to G2	Normal valve
0	6·5	85	1000
−0·1		71	
−0·2	4·1	60	500
−0·3			
−0·4	2·5	34·5	175
−0·5			
−0·6	1·2	13·0	50
−0·7			
−0·8	0·25	3·5	8
−0·9			
−1·0	0·1	1·0	2
−1·1	0·0	0·1	
−1·2			0·2
−1·3			

It will be observed that although the actual values of i_{vc} are so different, due to the influence of different space-charge conditions, nevertheless, the final cut-off values are approximately the same in each case.

The examples given so far in connection with the effect of variations of the i_{vc} with the grid-cathode distance indicate that large changes in i_{vc} can take place as the grid gap is varied. The order of the variations dealt with is, however, very much larger than we should ever encounter in any one type of valve due to variation in manufacturing tolerances, and it is desirable now to investigate how i_{vc} varies with smaller changes in the values of the grid-cathode distance.

The following experiments were carried out on small battery triodes. Their specification was as follows :—

Filament rating: 2 volts, 0·15 amp.
Filament coated diameter : 0″·0021.
Grid-filament clearance (normally 0″·029) (variable here).
Grid t.p.i.: 30.
Grid-wire diameter: 0″·004.
Grid-wire material: molybdenum.
Normal anode volts: 120.
Mutual conductance (at $v_a=120$, $v_f=2$, $v_g=0$): 1·6 ma./v.

Twelve such valves were assembled, all being alike apart from the grid-filament clearance. This clearance was varied by giving the grid different degrees of stretching on the jig. The distance between the grid wires internally from

one side to the other is referred to as the " grid gap " and is denoted by w. The normal grid gap for this valve was 0″·060.

The twelve valves were divided into four groups of three in each group, with $w = 0''{\cdot}050$, $0''{\cdot}060$ (the normal gap), $0''{\cdot}070$ and $0''{\cdot}080$ for the groups in order.

Table 8 gives the values of the initial velocity current in micro-amperes to the grid alone, also to grid and anode tied together, and V_r gives the respective retarding voltages required to reduce this current to zero.

Table 8

	1		2		3	
	i	V_r	i	V_r	i	V_r
Group 1 $w = 0''{\cdot}050$						
G only	4·6	−0·85	2·3	−0·83	4·1	−0·90
a+G	7·6	−0·82	3·7	−0·85	7·0	−0·86
Group 2 $w = 0''{\cdot}060$						
G only	Broken fil.		5·7	−0·83	4·3	−0·85
a+G			11·0	−0·95	7·6	−0·82
Group 3 $w = 0''{\cdot}070$						
G only	4·2	−0·83	0·50	−0·52	3·1	−0·87
a+G	8·0	−0·82	0·70	−0·50	5·3	−0·84
Group 4 $w = 0''{\cdot}080$						
G only	7·0	−0·93	3·2	−0·86	3·1	−0·80
a+G	13·2	−0·96	5·7	−0·96	5·7	−0·80

The figures given in table 8 indicate that there is no obvious connection between the initial velocity current and the grid gap over the small variations in grid gap which have been made here. Also it will be observed that the values of the retarding voltage do not follow the variations of the initial velocity current although there is a general statistical tendency for low values of i_{vc} to be associated with low values of V_r.

For a valve of this type, therefore, we can say that the static characteristics are not influenced to any appreciable extent by the variations in contact potential, initial velocity current, or the geometrical errors in assembly which are likely to occur on production.

It is desirable to record here also the mean values of anode current at $v_a = 120$, $v_g = 0$ and the average mutual conductance K of each group of valves having different values of w. These are given in table 9.

Table 9

Group	1	2	3	4
K	2·46	2·15	1·96	1·76
i_a	16·3	14·3	12·0	10·1

§7. CONTROL OF INITIAL VELOCITY CURRENT DURING PROCESSING

There are many factors which can affect the value of the contact potential between grid and cathode during processing. The more important of these may be summarized as follows—

Factors which give high initial velocity currents and low contact potential :	Factors which give low initial velocity currents and low contact potential :
(1) High lighting during activation on exhaust.	(1) Low lighting on exhaust.
(2) High E.C.H. schedule on exhaust.	(2) Low E.C.H. on exhaust.
(3) High lighting during ageing.	(3) Low lighting during ageing.
(4) Positive grid ageing.	(4) Grid volts zero or negative during ageing.
(5) High emission.	(5) Low emission due to poisoning or under-activation.
(6) Low emission due to over-activation.	(6) Running with filament lit but no positive potential applied to any other electrodes.

If the initial velocity current is brought up to a high value by positive grid ageing, it will often tend to fall again if the valve is run at zero grid- and normal anode-voltage. If, however, the anode voltage is kept low (about 30 volts), the initial velocity current will usually maintain its value and may even increase slightly.

In general, the initial velocity current has its highest value when the cathode has been slightly over-activated. It is possible to activate the cathode to such an extent that the emission has decreased considerably, but the initial velocity current may still have a high value.

There are many other factors which determine the magnitude of the initial current, such as smoothness of emitting surface, type of grid winding wire, and contamination of the wire before assembly. These factors are more concerned with the original design and assembly, and a discussion of these points would require an entirely separate paper.

§8. VARIATION OF INITIAL VELOCITY CURRENT DURING LIFE

If a valve has been stabilized by normal running after the ageing schedule, the initial velocity current will usually remain appreciably constant during the first 800 hours on life test. If the initial velocity current has been increased by high-temperature flash during or before ageing, it will also usually retain this higher value. If the initial velocity current has been reduced by running with filament lit, but no applied positive potential to any of the electrodes, then the initial velocity current will rise rapidly on life for the first hour and will take up a value equivalent to that which it would have had if the valve had been given normal running after normal ageing schedule. Therefore, reduction of initial velocity current by filament running alone is not a possible salvage schedule for valves showing too high initial velocity current.

If the initial velocity current on normal running is too high it is very difficult to give any treatment which will reduce this value in such a way that it will remain stable on life at the reduced value.

If the initial velocity current has been increased by high ageing schedule, or by flashing before or during the ageing schedule, it will usually retain this high initial velocity current during the subsequent normal run and during life. It is therefore quite feasible to give a salvage schedule to increase the initial velocity current and keep it stable, but it is often impossible to give a schedule which will reduce the initial velocity current.

If i_{vc} has been increased by positive grid ageing, there is a considerable probability that it will decrease when the valve is put on normal running again. Therefore a positive grid salvage schedule to increase the initial velocity current is not reliable.

§9. ACKNOWLEDGEMENT

In conclusion, I must thank Ferranti, Ltd., in whose laboratories these observations were made, for permission to publish this paper.

NOTE ON THE ENERGY ASSOCIATED WITH CAPILLARY RISE

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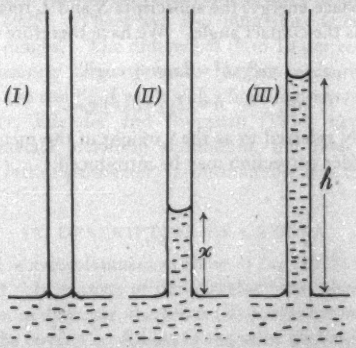
Received 22 *February* 1941

ABSTRACT. When a liquid rises in a capillary tube, energy is required not only to raise the column against gravity, but also to overcome the viscous forces. These two quantities of energy are shown to be equal. By equating their sum to the energy made available by the wetting of the walls of the tube, the usual capillary-rise equation is obtained.

It is obvious that the energy required to establish capillary elevation is derived from the diminution of surface energy which accompanies the movement of the liquid. It follows therefore that the equation relating the elevation with the surface tension and the radius of the tube may be established by equating the loss of free surface energy to the work done during the motion of the liquid. While it seems unlikely that this method of approach is entirely new, the writer



has not, so far, seen any reference to it, and the following analysis seems worth recording.

For the sake of simplicity we suppose that no part of the tube is submerged below the general level of the liquid, which is contained in a wide vessel. We then compare the energy at stage (I) (see figure), i.e. at the instant at which the tube touches the surface, with that at stage (III), when equilibrium has been reached.

The most obvious gain of energy is the potential energy of the raised column, which is equal to $\pi a^2 g\rho h^2/2$, where a is the radius of the tube ; but the problem is *not* solved by equating this to the loss of surface energy. Another (and, in fact, equal) quantity of work is done against the viscosity of the liquid. If the liquid were non-viscous it would oscillate continuously up and down the tube. When the liquid is viscous the oscillations, if they occur at all, are soon damped out, and kinetic energy is converted into heat. The amount of energy thus dissipated may be calculated by supposing that a force, F, acts downwards on the column as it moves from stage (I) to stage (III), the magnitude of F being just sufficient to prevent acceleration. In any intermediate stage, (II), F will be given by

$$F = \pi a^2 g\rho(h - x),$$

and the work done against F will be

$$\int_0^h F\,dx = \pi a^2 g\rho \int_0^h (h-x)\,dx = \pi a^2 g\rho h^2/2.$$

This is the energy which is converted into heat, and the total energy required to establish the capillary rise is obtained by adding it to the potential energy of the raised column. The two quantities of energy are equal, and their total is $\pi a^2 g\rho h^2$.

The loss of surface energy which occurs when an area $2\pi ah$ of the wall of the tube is wetted is $2\pi ah\,(\gamma_S - \gamma_{SL})$ or (by a well-known relation) $2\pi ah\gamma_L \cos\theta$, where γ represents free surface energy, the subscripts S and L refer to solid and liquid respectively, and θ is the contact angle. We have therefore

$$\pi a^2 g\rho h^2 = 2\pi ah\gamma_L \cos\theta,$$

$$\text{or} \qquad h = (2\gamma_L \cos\theta)/g\rho a.$$

If what is usually referred to as the " weight of the meniscus " is taken into account, the first-order correction may be introduced.

THE THERMAL EXPANSION OF ALUMINIUM FROM 0° TO 650° C.

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Received 5 *December* 1940. *Communicated by Prof. Sir W. Lawrence Bragg, F.R.S.*

ABSTRACT. The lattice spacing of aluminium has been measured as a function of temperature by means of a high-temperature Debye-Scherrer X-ray camera. The coefficient of expansion has been deduced. The results are:

Temp.	0°	100°	200°	300°	400°	500°	600°	650° C.
d	4·0391	4·0486	4·0592	4·0701	4·0820	4·0947	4·1087	4·1162
$10^6\,\alpha$	22·0	25·4	26·5	27·8	29·9	32·5	35·5	37·2

The systematic and random errors in d are each about 0·0001 A., the error in α from 1 to 2 per cent.

§1. INTRODUCTION

THE thermal expansion of aluminium has been investigated at the Reichsanstalt by Dittenberger (1902) and at the U.S. Bureau of Standards by Hidnert (1925). There are also numerous determinations covering shorter temperature ranges. The differences (5 to 10 per cent in the expansion coefficient) are sufficiently large to make further measurements of interest. This paper describes the results of work with a high-temperature Debye-Scherrer X-ray camera. Esser, Eilender and Bungardt (1938) have published X-ray results for several metals, including aluminium, but their accuracy (0·01 to 0·05 per cent in the lattice parameter) is considerably less than that attained in the present work.

§2. DESCRIPTION OF CAMERA

The camera used is a development of those of Jay (1933) and Hume-Rothery and Reynolds (1938). It consists of two circular brass plates, the upper (A in figure 1) about 19 cm. in diameter, the lower (B) somewhat larger and containing a groove to receive the cover C. A boss of the same diameter as the top plate is turned on the lower plate. The plates are held about 1·1 cm. apart by two brass V-blocks D and E. Each V-block carries a pair of stainless steel knife-edges which mark out definitely the length of film exposed to the X rays. The film, backed by a sheet of black paper, is held firmly against the upper plate and the boss of the lower plate by two strips of steel ribbon on each side. The strips are attached to the back V-block D and are clipped firmly to the front one E by

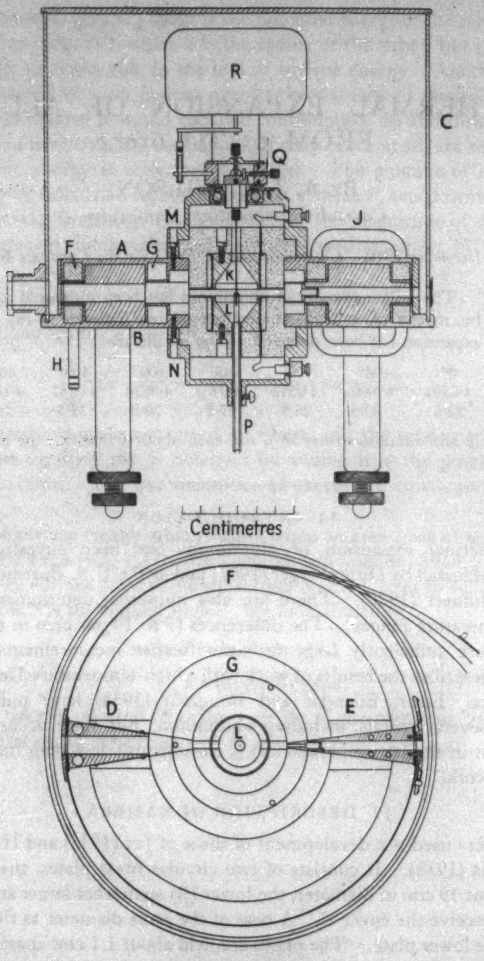

Figure 1. The camera.

phosphor-bronze springs. The lower diagram of figure 1 shows, on the left side of the camera, the film, paper and steel strips lying loosely; on the right side it shows them clipped to the front V-block.

Each plate contains two water-cooling channels F, G of about 1 cm. square section. The water enters through one of four tubes H at the back of the camera, passes through the V-block D to the upper plate, where it flows half-way round the outer channel till it meets a baffle, through the U-tube J to the inner channel, where it flows completely round and through another U-tube to the other half of the outer channel and finally emerges through another of the tubes H. The circulation in the lower plate is similar.

The furnace consists of two bobbins K, L of oxidation-resistant steel. The windings are of platinum insulated from the bobbins by mica. The bobbins are fastened by screws in two soapstone members held to the plates of the camera by circular brass clamps M, N. The leads from the furnaces are taken to small bakelite terminal boards on the clamps. The lower clamp N supports a platinum *vs.* platinum-rhodium thermocouple P which serves to measure the temperature of the furnace. The thermocouple is of the ring type described by Hume-Rothery and Reynolds (1938). The upper clamp M contains a ball-race carrying the device Q for centering the specimen. The specimen is contained in a thin-walled silica tube which is mounted in a brass rod screwed into a small brass block. This contains two mutually perpendicular grooves into which fit small discs borne on the ends of two screws. The specimen is easily brought into the axis of rotation of the camera by adjusting these screws. A third screw clamps the block in position after centering. The specimen is turned four times a minute by the small synchronous motor R, which can be swung back out of the way when it is necessary to remove the specimen holder. The leads from the motor and the upper half of the furnace pass out of the camera through four holes in the front V-block E.

The x rays enter the camera through a small hole in the front of the cover. Filters are put in a small pocket on the outside of the cover when necessary. The x rays pass through a hole in the block E and strike the slit system S, a small removable brass block fitting the inner end of the hole. The slit itself, about 0·8 by 3 mm., is at the outer end of this block; the wider part towards the centre of the camera prevents rays scattered by the edges of the slit from striking the film. The rays then pass out of the camera through a hole in the block D larger than that in the block E. Two strips fastened to the outside of D prevent any rays scattered by it from striking the film.

The heating currents in the two furnace bobbins can be varied independently, and at each temperature are adjusted so that the temperature distribution, when investigated by moving the thermocouple up and down, is symmetrical about the centre. Typical distributions are shown in figure 2. During the exposure the thermocouple is kept at the point marked T, and the reading corrected for the temperature difference between this point and the mean temperature of the effective part of the specimen. The temperature distribution is determined at the beginning of, twice during, and at the end of each exposure. At the higher

temperatures it is necessary to place an opaque screen between the furnace and the film to prevent fogging by the light emitted. The form of the temperature distribution depends somewhat on the material used for the screen. Black paper and aluminium foil have been used. Since aluminium foil reflects back much of the radiation from the furnace, the " centre drop " disappears when it is used. In every case the temperature distribution observed at the time is used in making the correction.

The furnace current is continually regulated by hand in order to keep the temperature constant. It rarely varies by more than 1° from the desired temperature, and the mean temperature during an exposure would differ from the

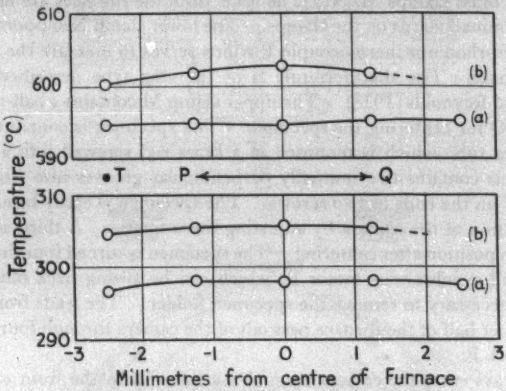

Figure 2. Temperature distribution in furnace.
T, position of thermocouple ; PQ, effective part of specimen.
(*a*) black paper; (*b*) aluminium foil between furnaces and film.

desired temperature by less than this, perhaps about 0°·5. The camera has not yet been used for work above 660° c., but was once maintained at 1000° c. for about half an hour without obvious damage.

§3. THE TEMPERATURE SCALE

Two platinum *vs.* platinum-rhodium thermocouples were used. Their e.m.f. was measured with a Cambridge Instrument Company Laboratory Thermocouple Potentiometer. Readings could be made to one microvolt without difficulty. The first thermocouple, protected by a silica tube, was calibrated by measuring its e.m.f. in melting and freezing tin, lead, zinc and sodium chloride. A curve was drawn of the deviation of its e.m.f. from the standard table given in the *International Critical Tables*. After a number of photographs of the aluminium specimen had been taken, short exposures were made near its melting point to determine the e.m.f. at which the reflections disappeared. This occurred about 6° higher than was expected. A similar test with lead showed its melting

point about 2° high. The temperatures of the exposures taken previously were recalculated using a deviation curve derived from the melting points of the lead and aluminium specimens. There was some possibility of the junction having been contaminated with zinc or aluminium, so a second thermocouple was made and calibrated in steam and at the melting points of lead, antimony and aluminium. The melting points of the metals were obtained by finding the e.m.f. at which their x-ray reflections disappeared. This method has the obvious advantage that conditions during calibration are the same as those during use. The e.m.f. could be determined within four microvolts. The melting points were taken to be

lead	327°·3 C.,
antimony	630°·5 C.,
aluminium	658°·9 C.

Nearly half the spacing measurements were repeated with the new thermocouple. The mean difference, after correction for unavoidable small differences in temperature, was $-0{\cdot}000\,1$ A., which, even if entirely due to errors in the temperature measurement, corresponds to less than a degree difference between the two temperature scales. It is thought that the temperature measurements were reproducible within 0°·5 and represent the International Temperature Scale within 1°.

Two objections have been raised against these temperature measurements. The first is that the temperature of the thermocouple is not that of the specimen. This may be true, but if so the difference is included in the calibration, since the temperature difference between the specimen and thermocouple is the same during calibration as during use. The second is that the temperature at which the x-ray reflections disappear might be higher than the melting point. This appears extremely unlikely. If the x-ray reflections from the liquid are the same as those of the solid, the liquid must have the same crystal structure and therefore the same physical properties as the solid.

§4. MEASUREMENT OF SPACINGS

The theory behind the use of this camera for the precise measurement of spacings has been considered fully by Bradley and Jay (1932). Some account of the practical details ought however to be given. As the camera is about twice the diameter of those described by Bradley and Jay, a piece of film encircling it completely would be of inconvenient size, so two pieces are used. To obtain S, the film length corresponding to four times the apparent angle of reflection, it is necessary to obtain the sum of three quantities:

(1) the distance between the back knife-edge and the line on the left half of the film,

(2) x, the arc length between the back knife-edges,

(3) the distance between the back knife-edge and the line on the right half of the film.

Measurements are made with an instrument made by the Cambridge Instrument Company to a National Physical Laboratory design. It is described fully in a paper by Sears and Turner (1941). The film is placed in it so that the scale reading of the shadow of the back knife-edge is $x/2$. The scale reading when the cross-hair is set on a line is then $x/2$ plus the distance between the shadow of the back knife-edge and the line. S is the sum of the scale readings of the line on the two halves of the film. S_k is the sum of the scale readings of the shadows of the front knife-edges on the two halves, and the apparent angle of reflection is $\theta = S\theta_k/S_k$.

The determination of the angles of reflection involves the knowledge of two constants of the camera, x and θ_k. These were obtained from actual measurement of the relevant parts of the camera. It has been usual (Bradley and Jay, 1933) in work with cameras of this type to use a camera angle chosen to make the a spacing of quartz 4·902 88 A., derived from the measurements of Bergqvist (1930). However, evidence has been accumulating which suggests that this is small by about one part in twenty thousand, so it appears preferable to use the angle obtained from measurement. There seems to be no difficulty in measuring the parts of the camera with accuracy sufficient to obtain the angle within about 0°·005, which corresponds to a systematic error in the lattice parameter of less than one part in fifty thousand.

The lattice parameter was calculated for each reflection above about 50°, plotted against $\sin^2\theta$ and extrapolated to $\sin^2\theta = 1$. To allow for refraction the

The spacing and expansion coefficient of aluminium

Temp.	Films.	d_{obs}	d_{calc}	Diff.	$10^6\partial d/\partial t$	$10^6\partial d/\partial t$	$10^6\alpha$	$10^6\alpha$
0° C.	4	$4{\cdot}0390_6$	$4{\cdot}0391_5$	$+0_9$	90·1	89	22·3	22·0
25°	5	$4{\cdot}0413_4$	$4{\cdot}0414_3$	$+0_9$				
100	1	$4{\cdot}0485_5$	$4{\cdot}0485_9$	$+0_4$	98·7	$102{\cdot}_7$	24·4	25·4
150°	1	$4{\cdot}0538_2$	$4{\cdot}0536_2$	-1_9				
200°	2	$4{\cdot}0591_5$	$4{\cdot}0588_9$	-2_6	107·3	$107{\cdot}_5$	26·4	26·5
300°	2	$4{\cdot}0700_5$	$4{\cdot}0700_6$	$+0_1$	116·0	113	28·5	27·8
400°	2	$4{\cdot}0819_8$	$4{\cdot}0820_9$	$+1_1$	124·6	$122{\cdot}_2$	30·5	29·9
500°	1	$4{\cdot}0946_8$	$4{\cdot}0949_8$	$+3_0$	133·3	133	32·6	32·5
600°	7	$4{\cdot}1086_7$	$4{\cdot}1087_4$	$+0_7$	141·9	146	34·5	35·5
650°	4	$4{\cdot}1161_8$	$4{\cdot}1159_4$	-2_4	146·2	153	35·5	37·2

The first values given for $\partial d/\partial t$ and α are from equation (2); the method of obtaining the others is discussed in the text.

spacings so found must be increased by an amount $(1-n)d$, where n is the refractive index of aluminium for the wave-length used. This correction is considered in more detail elsewhere (Edwards *et al.*, 1940; Wilson, 1940). For cobalt and copper radiations, those used in the present work, it amounts to 0·000 05 A. and 0·000 03 A. respectively.

About thirty exposures were made between 0° and 656°·$_3$ C. They were ordinarily as close to 0°, 100°, 200°, . . ., 600° as could conveniently be arranged. The spacings were corrected to the nearest even 100°. The temperature difference was ordinarily about 5°, so only a rough value of the expansion coefficient was needed. The spacings so obtained are given in the table. The number of films

on which each is based is given in column 2 of the table. Nearly all are averages of more than one film. The mean deviation from the mean of repeated spacings is 0·000 1 A. The spacing at 25°, 4·041 3_4 A., is in good agreement with the 4·041 3_9 A. of Jette and Foote (1935) and the 4·041 43 A. of Ievinš and Straumanis (1936). The aluminium used was said by the makers to be 99·992 per cent pure.

§ 5. CALCULATION OF THE EXPANSION COEFFICIENT

To determine the coefficient of expansion, $\alpha = \frac{1}{d}\frac{\partial d}{\partial t}$, it is first necessary to obtain the derivative of d with respect to the temperature t. This is a matter of some difficulty. The values of d are nearly a parabolic function of t. By the method of least squares

$$d = 4{\cdot}039\,1_5 + 0{\cdot}009\,00_8\left(\frac{t}{100}\right) + 0{\cdot}000\,431_7\left(\frac{t}{100}\right)^2 \qquad \ldots\ldots(1)$$

was found to be the best parabolic equation. It shows systematic deviations from the experimental points which are probably significant (columns 4 and 5 of the table). The slope of this equation is

$$10^6\frac{\partial d}{\partial t} = 90{\cdot}0_8 + 8{\cdot}63_4\left(\frac{t}{100}\right). \qquad \ldots\ldots(2)$$

Values are given in column 6 of the table. The author is, however, of the opinion

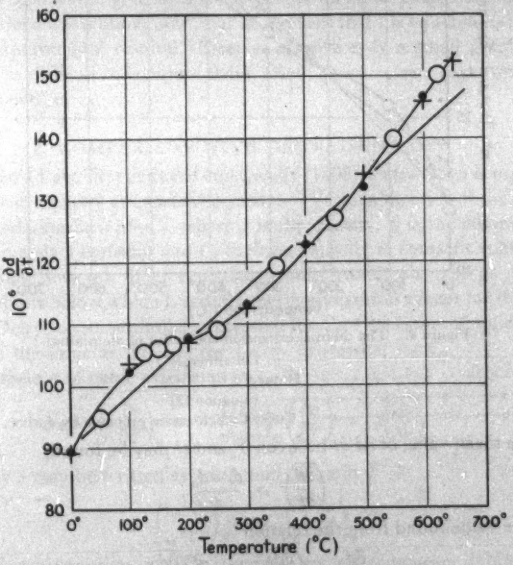

Figure 3. Slope of spacing-temperature curve.
Straight line, equation (2); for explanation of points see text.

that this equation does not give the best possible representation of $\partial d/\partial t$. It is plotted in figure 3 for comparison with values of $\partial d/\partial t$ obtained in other ways.

(1) The mean value of $\partial d/\partial t$ over intervals of 50° or 100° may be obtained by subtracting the values of d in the table and dividing by the temperature difference. These means are plotted as circles.

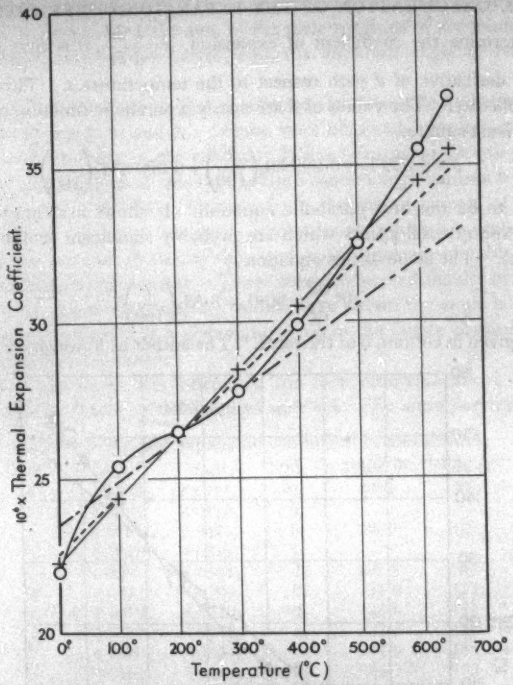

Figure 4. The thermal expansion coefficient of aluminium.

—— — —— — —— — —— Dittenberger ;

— — — — — — — — — Hidnert ;

x——x——x——x——x Equation (2) ;

o——o——o——o——o Curve thought more probable by author.

(2) The mean value of $\partial d/\partial t$ between 0° and t° may be found,

$$\left(\overline{\frac{\partial d}{\partial t}}\right) = \frac{d_t - d_0}{t}, \qquad \ldots\ldots(3)$$

and the true value found from the relation

$$\frac{\partial d}{\partial t} = \left(\overline{\frac{\partial d}{\partial t}}\right) + t\frac{\partial}{\partial t}\left(\overline{\frac{\partial d}{\partial t}}\right). \qquad \ldots\ldots(4)$$

The term $t\frac{\partial}{\partial t}\overline{(\partial d/\partial t)}$ is fairly small compared with $\overline{\partial d/\partial t}$, so it introduces less uncertainty into the value of $\partial d/\partial t$ than there would be in the value of $\partial d/\partial t$ found directly. Values of $\partial d/\partial t$ from equation (4) are plotted as crosses.

(3) Successive groups of three values of d may be fitted by parabolas and the slope at the first, second and third temperatures plotted. These values are plotted as large dots.

The three methods agree in giving an S-shaped curve for $\partial d/\partial t$. This has been drawn in figure 4, and values read from it are given in column 7 of the table. It has not been thought worth while to reproduce the details of the calculations. Anyone interested should have little difficulty in repeating them.

Finally, the values of $\partial d/\partial t$ are divided by the corresponding values of d to give α, the true expansion coefficient. Column 8 gives α from equation (2), column (9) from the other methods. The latter is believed to be a better representation of the expansion coefficient of aluminium. In figure 4 the two sets of values are plotted for comparison with the values of α calculated from the equations of Dittenberger (1902) and Hidnert (1925). It will be seen that Hidnert's results agree closely with the values of α from equation (2). Unfortunately, he does not give the experimental results on which his equation is based, so it is not possible to see if they also would give an S-shaped curve on closer examination. Dittenberger's results are low above 300°, but he remarks that his equation is not a good fit for his experimental results. Esser *et al.* give only a small graph of their results, so it is difficult to compare them. Both d and α seem high, the latter by 8 per cent at 600° C.

§ 6. COMPARISON WITH GRÜNEISEN'S LAW

Grüneisen's Law, first deduced empirically (1908), states that α is proportional to C_p, the heat capacity at constant pressure. Various theories of the solid state lead to the relation $3\alpha=\rho\beta\gamma C_v$, where ρ is the density, β is the compressibility, γ is approximately a constant and C_v the heat capacity at constant volume. This relation, which does not differ essentially from Grüneisen's, may be derived by a method set out below which is strictly thermodynamical except for the assumption of the Debye or some similar expression for C_v. Its limits of validity should therefore be the same as those of the Debye expression for C_v.

The coefficient of cubic expansion is

$$3\alpha=\frac{1}{v}\left(\frac{\partial v}{\partial T}\right)_p=-\frac{1}{v}\left(\frac{\partial v}{\partial p}\right)_T\left(\frac{\partial p}{\partial T}\right)_v=\beta\left(\frac{\partial s}{\partial v}\right)_T. \qquad \ldots\ldots(5)$$

The entropy s may be written as a function of v and T :—

$$s=\int_{v_0}^{v}\left(\frac{\partial s}{\partial v}\right)_T dv+\int_0^T\left(\frac{\partial s}{\partial T}\right)_v dT, \qquad \ldots\ldots(6)$$

where the first integration is at $T=0$ and the second at constant volume v; v_0 is

the volume at $p=T=0$. Since $\alpha \to 0$ and β remains finite when $T \to 0$, the first integral vanishes (equation (5)). In the second integral $(\partial s/\partial T)_v = C_v/T$, so

$$s = \int_0^T \frac{C_v}{T} dT, \qquad \ldots\ldots(7)$$

and, therefore,

$$3\alpha = \beta \left(\frac{\partial}{\partial v}\right)_T \int_0^T \frac{C_v}{T}\, dT. \qquad \ldots\ldots(8)$$

The Einstein and Debye theories of heat capacity lead to the result that $C_v(v, T) = C_v(\theta/T)$, where θ is a function of v only. Substituting in (8) gives

$$\begin{aligned} 3\alpha &= \beta \int_0^T \frac{d\theta}{dv} \frac{C_v'(\theta/T)}{T^2}\, dT = -\frac{1}{v}\, \beta \frac{d \log \theta}{d \log v} \int_\infty^T C_v'(\theta/T)\, d(\theta/T) \\ &= \rho\beta\gamma C_v, \qquad \ldots\ldots(9) \end{aligned}$$

on integrating and putting $\gamma = -d \log \theta / d \log v$, $\rho = 1/v$.

Neither the "law" $\alpha = \text{constant} \times C_p$ nor $3\alpha = \rho\beta\gamma C_v$ is well obeyed at high temperatures. Kelley (1934) gives the heat capacity of aluminium at 0° C. as 5·68 cal./gram atom ° C., at 600° as 7·61 cal./gram atom ° C. The ratio α/C_p at 0° is then 3·88, at 600° 4·62. It may be concluded, therefore, since the remainder of the argument is purely thermodynamical, that the assumptions on which the Debye theory is based are not satisfied by aluminium in the temperature range covered by this work.

§ 7. ACKNOWLEDGEMENTS

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THE CALIBRATION OF DEBYE-SCHERRER X-RAY POWDER CAMERAS

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ABSTRACT. Quartz has been widely used as a standard for the calibration of Debye-Scherrer x-ray powder cameras, and hence for the measurement of crystal lattice constants in terms of Siegbahn's wave-length scale. The quartz standard was first proposed by Bradley and Jay, who assumed the spacing derived from Bergqvist's measurements, and determined the axial ratio, and hence c, by means of the camera designed by Bradley.

A later direct determination of c by Elg, however, is not consistent with the value of c as derived above. The present paper describes a determination of both a and c in terms of the wave-length scale, using Debye-Scherrer cameras. The results, $a=4{\cdot}903\,2$ A. and $c=5{\cdot}393\,7$ A. at 18° C., agree very closely with the value of c obtained by Elg and the axial ratio of Bradley and Jay, but indicate that Bergqvist's value of a is low by about one part in 20 000.

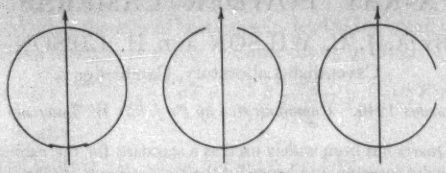
The revised values of the unit cell dimensions should be adopted when quartz is used to calibrate a camera, so a table of angles of reflection is given. Our results indicate, however, that a higher accuracy can be obtained by direct measurement of the camera.

§1. INTRODUCTION

THREE main ways have been proposed for deriving lattice spacings to an accuracy of about one part in 30 000 from powder photographs. These are illustrated in figure 1, in which the circular arc represents the film and the arrow represents the direction of the x rays. The method of Bradley and Jay (1932) permits a camera of very simple construction and manipulation, but the method of van Arkel (1926) allows a more direct measurement of the angles of the high-order lines, since they are all close together on the film. This is true also of the " asymmetric " method of Ievinš and Straumanis (1936). In the Bradley-Jay method, the high-order angles are measured by the interposition of knife edges, and thus it is necessary that the angle subtended by these knife edges at the centre of the camera should be known. This is somewhat of a disadvantage. In 19 cm.-diameter cameras (Bradley, Bragg and Sykes, 1940), however, the other two methods require an inordinate length of film, whereas the Bradley-Jay camera can easily be adapted to take two films of moderate length by introducing another set of knife edges opposite the first. The Straumanis method would require film at least 50 cm. long. The van Arkel method could be used with

shorter film, but at the expense of omitting part of the x-ray pattern. Thus for general utility, in which lattice spacings are not the only consideration, the Bradley-Jay arrangement is probably best.

The calibration of cameras is therefore of importance. Bradley and Jay (1933) have discussed the matter and have decided that the best method is to use a calibrating substance whose cell dimensions are accurately known. They chose



(*a*) Bradley and Jay. (*b*) van Arkel. (*c*) Straumanis.

Figure 1. Arrangements of film in Debye-Scherrer cameras. In (*a*) the knife edges are shown.

quartz as this substance, but, unfortunately, evidence has been accumulating that the *a* spacing of quartz determined by Bergqvist (1930), which they used as a basis, is low by about one part in 20 000. The evidence may be summarized as follows :

(1) Bradley and Jay were able to determine the c/a ratio from their photographs, and hence to assign a value to *c* based on Bergqvist's value for *a*. The length of the *c* axis so found does not agree with the direct measurement made by Elg (1937). In order to make clear the divergence, the results are collected in table 1, the italicized number in each row being derived from the other two.

Table 1. Previous measurements of the lattice constants of quartz. (Values in italics are deduced from the other values in the same horizontal line.)

a	*c/a*	*c*
4·902 88 (Berg.)	1·100 02 (B–J)	*5·393 27*
4·902 88 (Berg.)	*1·100 09*	5·393 62 (Elg)
4·903 20	1·100 02 (B–J)	5·393 62 (Elg)

(2) When the angle of a camera is found by direct measurement it is always lower by about 0°·04 than that derived from calibration with quartz, by the use of Bradley and Jay's table of angles of reflection (Bradley and Jay, 1932).

(3) The method recommended by Bradley and Jay is to determine the camera angle, θ_k, from each high-order reflection on a quartz photograph. If S_k is the length of film exposed between the knife edges, and S is the length between the two lines produced by a reflection whose Bragg angle is θ, then $\theta_k = \theta S_k/S$. Since the lines on the film are displaced from their theoretical positions by absorption and error in the position of the axis of rotation of the camera, the values of θ_k so found will not be constant. Bradley and Jay have shown that

these errors make θ_k a linear function of sin $2\theta/2\theta$, and, therefore, that the true value of θ_k is found by plotting its apparent value against sin $2\theta/2\theta$ and extrapolating to $\theta = 90°$. Quartz with copper radiation gives no reflections above 80°, so that the extrapolation is rather long. Cobalt radiation gives reflections at higher angles, but if the angles of these be calculated from the axes adopted by Bradley and Jay, the extrapolation is curved upwards above 82°. The curvature could be explained by an error in the axes.

§ 2. MEASUREMENTS OF THE SPACINGS OF QUARTZ

Since it had been found that the previously reported values of a, c and c/a are inconsistent, it was decided to attempt direct measurement of a and c. Two methods and two cameras have been used. The methods were those of van Arkel and of Bradley and Jay ; the cameras were no. 8, a 19 cm. diameter camera used in many of the investigations of Bradley and his collaborators, and no. 10, a high-temperature camera of the same diameter.

(a) *The van Arkel method.* Two photographs were taken with each of chromium, cobalt and copper radiations in camera 10. The values of a were calculated from lines of low l index, c from lines of high l index, an approximate value of the axial ratio being used. The spacings were plotted against $\cos^2\theta$ and extrapolated to $\theta = 90°$. The results obtained are given in table 2. They have been corrected for refraction (Edwards, Lipson and Wilson, 1940; Wilson, 1940) and temperature. The wave-lengths of the various radiations were taken as:

chromium, $K\alpha_1 = 2{\cdot}285\ 03$ A., $K\alpha_2 = 2{\cdot}288\ 91$ A.,
cobalt, $K\alpha_1 = 1{\cdot}785\ 29$ A., $K\alpha_2 = 1{\cdot}789\ 19$ A.,
copper, $K\alpha_1 = 1{\cdot}537\ 395$ A., $K\alpha_2 = 1{\cdot}541\ 232$ A.

(b) *The Bradley-Jay method.* The angles of the cameras were measured by two methods. The first consisted in measuring the diameter of the camera and the distance between the knife edges and calculating the corresponding angle. The diameter was measured to 0·01 cm., corresponding to 0°·002 in the camera angle, the distance between the knife edges (~3 cm.) to 0·002 cm., corresponding to 0°·003 in the camera angle. The second method consisted in centering the camera on the table of a spectrometer and measuring directly the angle subtended by the knife edges. This angle (which is four times the complement of θ_k) was measured within 2′, corresponding to 0°·008 in θ_k. The results obtained were 86°·102 and 86°·102 for camera 8, 86°·693 and 86°·691 for camera 10. Values of a and c from photographs taken in both cameras are given in table 2.

The results in table 2 are in the order in which the films were taken. To show more readily the agreement between the different radiations, film arrangements and cameras, table 3 has been prepared. The mean values of a found by the two methods used differ from each other by one part in 60 000, those of c by one part in 100 000. It is clear therefore that the Bradley and Jay method,

by the use of measured camera angles, agrees, within the limits of accuracy, with the van Arkel method, which does not require a precise knowledge of any camera constant. The values of a and c with the different radiations agree with each other to the same order of accuracy as the wave-lengths are given. This

Table 2. The spacings of quartz at 18° C.

Film	Method	Camera	Radiation	a	c
873	B–J	10	Cu	*	$5{\cdot}393\ 5_8$
876	B–J	10	Cu	*	$5{\cdot}393\ 6_6$
877	B–J	10	Cu	*	$5{\cdot}393\ 6_5$
935	B–J	10	Cr	$4{\cdot}903\ 1_2$	$5{\cdot}393\ 7_0$
937	B–J	10	Cr	$4{\cdot}903\ 1_1$	$5{\cdot}393\ 8_3$
1787	vA	10	Co	$4{\cdot}903\ 2_0$	$5{\cdot}393\ 6_8$
1788	vA	10	Co	$4{\cdot}903\ 1_8$	$5{\cdot}393\ 7_3$
1789	vA	10	Cr	$4{\cdot}903\ 2_1$	$5{\cdot}393\ 8_1$
1795	vA	10	Cr	$4{\cdot}903\ 2_5$	$5{\cdot}393\ 6_8$
1813	vA	10	Cu	$(4{\cdot}903\ 4_3)$	$5{\cdot}393\ 7_9$
1814	vA	10	Cu	$(4{\cdot}903\ 1_8)$	$5{\cdot}393\ 7_4$
1981	B–J	8	Co	$4{\cdot}903\ 1_2$	$5{\cdot}393\ 6_4$
1982	B–J	8	Co	$4{\cdot}903\ 2_6$	$5{\cdot}393\ 8_6$
1983	B–J	8	Cr	$4{\cdot}903\ 0_9$	$5{\cdot}393\ 6_8$
2022	B–J	8	Cu	$(4{\cdot}903\ 2_7)$	$5{\cdot}393\ 6_4$

* Copper radiation is not really suitable for the determination of a. On these films (originally taken for calibrating camera 10) an insufficient number of lines had been measured to make an extrapolation curve.

is some evidence of the absence of systematic error, for the high-order reflections with the different radiations are at quite different angles. The agreement between the two cameras is further evidence of the absence of systematic error. It is difficult to believe that the error in the mean of all values of a and c exceeds

Table 3. The spacings of quartz at 18° C. Averages to show absence of systematic error

Films averaged	a	c	c/a
Bradley and Jay method	$4{\cdot}903\ 1_8$	$5{\cdot}393\ 6_9$	1·100 04
van Arkel method	$4{\cdot}903\ 2_4$	$5{\cdot}393\ 7_4$	1·100 04
Chromium radiation	$4{\cdot}903\ 1_6$	$5{\cdot}393\ 7_4$	1·100 05
Cobalt radiation	$4{\cdot}903\ 1_9$	$5{\cdot}393\ 7_3$	1·100 05
Copper radiation	$(4{\cdot}903\ 2_9)$	$5{\cdot}393\ 6_8$	(1·100 01)
Camera 8	$4{\cdot}903\ 1_8$	$5{\cdot}393\ 7_0$	1·100 04
Camera 10	$4{\cdot}903\ 2_1$	$5{\cdot}393\ 7_1$	1·100 04
All films	$4{\cdot}903\ 2_0$	$5{\cdot}393\ 7_1$	1·100 04

0·000 1 A., one part in 50 000. As some sources of error will affect a and c in the same direction, the error in the mean axial ratio is probably less than this, perhaps one part in 100 000.

Those who have used quartz as a standard will wish to know by how much their spacing measurements are likely to be wrong. It might at first be thought

that the error would be the same as that in quartz, about 0·007 per cent, but the following considerations show that the correction is not quite so simple. The error in spacing δa produced by any error in angle $\delta\theta$ is given by $\delta a/a = -\cot\theta\,\delta\theta$ (Bradley and Jay, 1932). If the error in θ_k is $\delta\theta_k$, the error in θ is $\theta\,\delta\theta_k/\theta_k$, so that the error in the spacing is $-\theta\cot\theta\,\delta\theta_k/\theta_k$. This has been plotted in figure 2 as a function of $\cos^2\theta$ for $\theta_k = 85°$, $\delta\theta_k = 0°{\cdot}04$. The spacing is low by an amount which vanishes for $\cos^2\theta = 0$, but the error is not linear in $\cos^2\theta$. The error due to wrong camera angle is therefore only partially eliminated by the usual extrapolation procedure, and the amount left will depend on the position of the highest order line. Straight-line extrapolations have been drawn in the figure for highest order lines at 70°, 75° and 80°. It appears that the residual errors in a are about

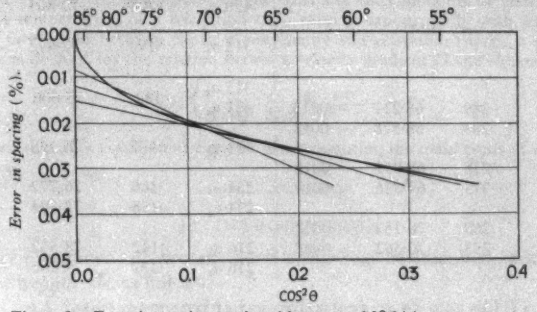


Figure 2. Error in spacing produced by error of 0°·04 in camera angle.

0·015, 0·012, 0·009 per cent respectively. These figures are given to show the variation of the correction only; it is not suggested that they should be used to correct existing spacing measurements.

§ 3. THE ANGLES OF REFLECTION FROM QUARTZ

On the basis of this work it is recommended that when possible the angles of Bradley-Jay type cameras be measured directly. However, some existing cameras cannot be taken to pieces for this purpose, and those who have used the method may wish to derive angles from existing measured photographs. It seems desirable, therefore, to give a table of the angles of reflection of quartz calculated from the new values of the axes. Table 4 gives, in addition to the information provided in Bradley and Jay's original table, the rate of change of each angle with temperature. This is useful when the temperature at which the calibration photographs are taken is not exactly 18° C. As an illustration of the order of accuracy obtained it may be said that the values of the angles of camera 10 obtained by the use of this table from films 873, 876 and 877 are 86°·687, 86°·696 and 86°·708 respectively. These are not very concordant, but the measured angle lies within their range.

Table 4. The angles of reflection for quartz. Cu radiation, 18° c.

Line	$\frac{\sin 2\theta}{2\theta}$	θ	$\partial\theta/\partial t$	Line	$\frac{\sin 2\theta}{2\theta}$	θ	$\partial\theta/\partial t$
412 α_1	·394	61·304°	−·0013	502 α_1 }	·239	71·632°	−·0020
412 α_2	·390	61·567	−·0014	225 α_1 }			
305 α_1	·358	63·631	−·0012	502 α_2 }	·233	72·065	−·0022
305 α_2	·354	63·920	−·0012	225 α_2 }			
				331 α_1 }			
116 α_1	·328	65·613	−·0011				
116 α_2	·324	65·931	−·0011	331 α_2	·226	72·509	−·0024
501 α_1 }				420 α_1	·215	73·319	−·0026
501 α_2 }	very faint			420 α_2	·208	72·803	−·0026
404 α_1 }				315 α_1	·190	72·092	−·0022
404 α_2 }							
				315 α_2 }	·182	75·606	−·0026
206 α_1	·289	68·217	−·0013	421 α_1 }			
206 α_2	·284	68·578	−·0013				
				421 α_2	·175	76·138	−·0031
413 α_1	·278	68·951	−·0018				
413 α_2	·273	69·326	−·0019	234 α_1	·166	76·772	−·0028
				234 α_2	·158	77·394	−·0030
330 α_1	·260	70·162	−·0021				
330 α_2	·255	70·562	−·0022	216 α_1	·142	78·552	−·0026
				216 α_2	·132	79·280	−·0028

§ 4. ACKNOWLEDGEMENTS

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NOTE ON VISCOUS AND PLASTIC FLOW

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ABSTRACT. On the assumption of the existence of a modulus of elasticity independent of temperature, of a relaxation time varying exponentially with temperature, and of an interaction between elastic strain energy and activation energy, a generalized expression is derived for the relation between velocity gradient D and shearing stress S as follows :—

$$S=D\,.\,GAe^{\frac{(S_0-S)^2V}{2GRT}}.$$

It is shown that this equation is capable of representing the main types of viscous and plastic flow.

§1. INTRODUCTION

"The phenomena of viscosity in all bodies may be described, independently of hypothesis, as follows :—

"A distortion or strain of some kind which we may call S is produced in the body by displacement. A state of stress or elastic force which we may call F is thus excited. The relation between the stress and the strain may be written $F=ES$, where E is the coefficient of elasticity for that particular kind of strain. In a solid body free from viscosity F will remain equal to ES and

$$\frac{dF}{dt}=E\frac{ds}{dt}.$$

If, however, the body is viscous, F will not remain constant, but will tend to disappear at a rate depending on the value of F and on the nature of the body. If we suppose this rate proportional to F, the equation may be written

$$\frac{dF}{dt}=E\frac{dS}{dt}-\frac{F}{T},$$

which will indicate the phenomena in an empirical manner. For if S be constant,

$$F=ESe^{-t/T},$$

showing that F gradually disappears, so that if the body is left to itself it gradually loses any internal stress, and the pressures are finally distributed as in a fluid at rest.

"If dS/dt is constant, that is, if there is a steady motion of the body which continually increases the displacement,

$$F = ET\frac{ds}{dt} + Ce^{-t/T},$$

showing that F tends to a constant value depending on the rate of displacement. The quantity ET by which the rate of displacement must be multiplied to get the force may be called the *coefficient of viscosity*. It is the product of a coefficient of elasticity, E, and a time T, which may be called the *time of relaxation* of the elastic force. In mobile fluids T is a very small fraction of a second, and E is not easily determined experimentally. In viscous solids T may be several hours or days, and then E is easily measured. It is possible in some bodies that T may be a function of F, and this would account for the gradual untwisting of wires after being twisted beyond the limit of perfect elasticity. For if T diminishes as F increases, the parts of the wire furthest from the axis will yield more rapidly than the parts near the axis during the twisting process, and when the twisting force is removed the wire will at first untwist till there is equilibrium between the stresses in the inner and outer portions. These stresses will then undergo a gradual relaxation; but since the actual value of the stress is greater in the outer layers, it will have a more rapid rate of relaxation, so that the wire will go on gradually untwisting for some hours or days, owing to the stress on the interior portions maintaining itself longer than that of the outer parts. This phenomenon was observed by Weber in silk fibres, by Kohlrausch in glass fibres, and by myself in steel wires" (Maxwell, 1867).*

In the above statement Maxwell gives a masterly and concise analysis of the meaning of viscosity, and it would be difficult to improve on his wording. He implies that the differences between the elastic behaviour of a solid and a liquid are due to differences in relaxation time and not in elastic modulus, and that if in a liquid the time of observation could be made small in comparison with the time of relaxation, it would be possible to determine a value of the elastic modulus identical with that of a viscous solid. He also suggests that the time of relaxation may well be a function of the force.

The significance of these observations seems to have escaped attention, and it has remained for W. Kühn (1939) to show how a constant value of elastic modulus together with a plurality of relaxation times can account for the elastic behaviour of high polymers and of amorphous materials. The reality of this constancy of elastic modulus has recently received considerable experimental support from the work of C. V. Raman and C. S. Venkateswaran (1939), who have found that, for mechanical disturbances of sufficiently high frequencies, a liquid behaves essentially as a solid, and also from the work of Hartshorn, Megson and Rushton (1940), who have found that the viscosity of a number of thermoplastic resins is numerically equal to the product of their electrical relaxation time and a quantity of the same order of magnitude as the modulus of rigidity

* The symbols in the above quotation are those of Maxwell, and do not correspond with those used below.

of such materials in the solid state. The work of M. Kornfeld and M. Ryvkin (1940) on the brittleness of liquids gives striking support to the existence of rigidity in liquids at high rates of deformation.

In this note I wish to enlarge on Maxwell's suggestion that the time of relaxation may be a function of the stress.

§ 2. THE ACTIVATION ENERGY OF VISCOUS FLOW

Analysis of the results of Hartshorn. Megson and Rushton (1940) shows that the mean relaxation time (seconds) of their phenol, cresol and xylenol formaldehyde resins varies with the absolute temperature T according to the relation

$$\lambda = Ae^{U/RT}, \qquad \ldots\ldots(1)$$

where A and U are constants characteristic of the resin and $\boldsymbol{R}$ is the gas constant.

Such an equation has already been suggested by Frenkl, and in the light of his analysis U can be interpreted as the activation energy required to shift an atom or molecule from its normal position (Frenkl, 1926). The numerical values of A and U are as follows:—

Resin	A in seconds	U in joules per gram mol.
Phenol-formaldehyde	$5{\cdot}37\times10^{-60}$	$3{\cdot}54\times10^{5}$
o-cresol-formaldehyde	$2{\cdot}40\times10^{-60}$	$3{\cdot}31$,,
p-cresol-formaldehyde	$2{\cdot}82\times10^{-51}$	$2{\cdot}90$,,
m-cresol-formaldehyde	$3{\cdot}89\times10^{-50}$	$3{\cdot}04$,,
m-5-xylenol-formaldehyde	$1{\cdot}74\times10^{-48}$	$3{\cdot}00$,,

§ 3. DETERMINATION OF ACTIVATION ENERGY FROM VISCOSITY

Since according to Maxwell the viscosity is the product of the elastic modulus and the time of relaxation, equation (1) can equally well be written

$$\eta = GAe^{U/RT}. \qquad \ldots\ldots(2)$$

This is, of course, Andrade's equation (1930). From the viscosity determinations of Hartshorn, Megson and Rushton, we get the following values for U:

Resin	U in joules per gram mol.
Phenol formaldehyde	$3{\cdot}41\times10^{5}$
o-cresol-formaldehyde	$2{\cdot}92$,,
p-cresol-formaldehyde	$3{\cdot}19$,,
m-cresol-formaldehyde	$3{\cdot}13$,,
m-5-xylenol-formaldehyde	$3{\cdot}03$,,

It will be seen that the values of U given by viscosity measurements agree well with those given by measurements of relaxation time, and this agreement is a confirmation that Maxwell's suggestion is true for these resins. R. Houwink (1934) has given values of $U/\boldsymbol{R}$ for a number of materials, and it is of interest to compare his results with those of Hartshorn, Megson and Rushton.

Material	U/R	U in joules per gram mol.
Alkaline tri-cresol formaldehyde resin	26000	$2{\cdot}12\times10^5$
,, *o*-cresol formaldehyde resin	21000	1·74 ,,
,, *m*-cresol ,, ,,	27000	2·24 ,,
,, *p*-cresol ,, ,,	25000	2·08 ,,
,, phenol ,, ,,	32000	2·66 ,,
Acid phenol formaldehyde resin	24000	1·95 ,,
Cumarone resin	40000	3·32 ,,
Glyptal resin	19000	1·58 ,,

The values for U are of the same order of magnitude as those determined by Hartshorn, Megson and Rushton.

§4. THE PHYSICAL SIGNIFICANCE OF U

The above results suggest that the deformation under stress of these resins takes place by a change of molecular position from one stable state, over an energy barrier U, to another stable state, and that only those molecules (or groups of molecules) having sufficient energy of thermal agitation to get over the barrier are able to make the change.

From the values of relaxation times, Hartshorn, Megson and Rushton conclude that it is only the OH groups that determine the mean relaxation time, so that the process of deformation is possibly a process of slipping of the OH groups over one another, and that the energy of activation U for these resins is the energy required to cause slipping between the OH groups.

Since U is a measure of the energy of the bond per gram molecule, we can deduce from it the maximum cohesive strength of the material by equating the activation energy per unit volume to the work done on a unit volume in straining the material until the stress reaches its maximum value S_0. Let V be the volume of a gram mol. Then

$$\frac{U}{V}=\frac{1S_0^2}{2E},$$

where E is the value of Young's Modulus.

For the phenol formaldehyde resin investigated by Hartshorn, Megson and Rushton (assuming a molecular weight of 500) we get $V=500/1{\cdot}29$ c.c. or $30{\cdot}5/1{\cdot}29$ cu. inches, and putting $E=0{\cdot}536\times10^6$ lb./in.^{2}, we find $S_0=3{\cdot}7\times10^5$ lb./in.^{2} or $2{\cdot}6\times10^4$ kg./cm.^{2}

This is of the order of magnitude of the ultimate cohesive strength of other materials.

A molecular weight of 500 corresponds to about 5 OH groups per gram mol. The activation energy per OH group of the phenol formaldehyde resin will thus be 16·9 kilocalories. This is of the correct order of magnitude [see Harms (1939)].

§ 5. INTERACTION BETWEEN STRAIN ENERGY AND ACTIVATION ENERGY

The activation energy arises from the attractions between the molecules; it is potential energy and unaffected by temperature. When therefore the material is strained there must be (neglecting thermal effects) a decrease in the height of the energy barrier corresponding to the gain in potential energy (strain energy) by the molecules. Suppose we apply an external shear stress of S units per unit area, then the strain energy stored up per unit volume will be $\frac{1}{2}S^2/G$ and the energy barrier (or activation energy) will be

$$\frac{1}{2G}(S_0-S)^2V,$$

where V is the volume of a gram mol., G is the modulus of rigidity and S_0 the ultimate cohesive stress as explained above [cf. R. Becker (1926)].

Equation (1) consequently becomes

$$\lambda = Ae^{\frac{(S_0-S)^2V}{2GRT}}. \qquad \ldots\ldots(3)$$

Let the velocity gradient be denoted by D: then under a constant shear stress S we see from Maxwell's analysis (quoted in the introduction) that

$$S = GD\lambda. \qquad \ldots\ldots(4)$$

$$\therefore \quad S = GDAe^{\frac{(S_0-S)^2V}{2GRT}}. \qquad \ldots\ldots(5)$$

Equation (5) gives a general relation between velocity gradient and shear stress. In order to appreciate its significance it is convenient to write it in the form

$$x = ye^{(1-x)^2/p}, \qquad \ldots\ldots(6)$$

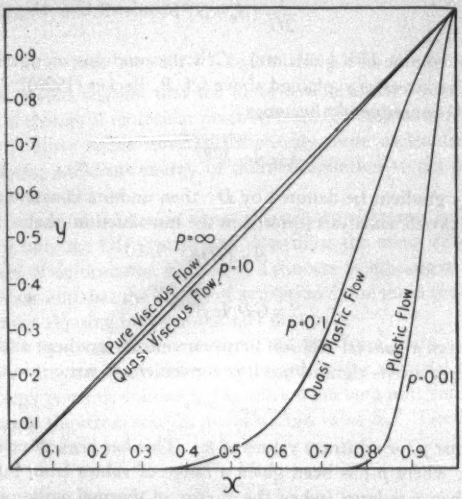
and plot x against y for arbitrary values of p. This has been done in the accompanying figure, where p has been given a range of values from infinity to 0·01. We see that when p is large (when the energy of thermal agitation is high) the relation between p (the velocity gradient) and x (the shear stress) is linear. This corresponds to pure viscous flow as observed in water and other simple liquids. In such materials only pure viscous flow can be observed, because if we endeavour to decrease the temperature, solidification takes place; on the other hand, we are unable to apply any appreciable shearing stress because of the enormously high velocities that would result. Thus W. Kühn (1939) has pointed out that a shearing stress of only 1 gram/cm^2 in water at 20° c. on a plane distant 1 cm. from a fixed parallel boundary would, in the absence of turbulence, cause the plane to move with a velocity of 10^5 cm./sec. or over 2000 m.p.h. The relaxation time of water at this temperature is of the order of 10^{-12} seconds.

In the case of liquids made of large molecules, however, it is possible to decrease the temperature without solidification, and such liquids very often show

quasi-viscous flow corresponding to the curve of $p = 10$. The velocity gradient is no longer proportional to the shearing stress and the viscosity decreases as the stress increases.

If it is possible to decrease the value of p to 0·1 the liquid acquires a yield point which becomes more and more definite as p decreases. These curves correspond to plastic flow.

I would like to emphasize in conclusion that this note is intended only to show how interaction between strain energy and activation energy can account for some of the anomalous flow characteristics of amorphous materials. There



Curves showing change from pure viscous or Newtonian flow to plastic or Bingham flow as the temperature is reduced. In viscous flow there is no yield point, but in plastic flow there is an apparent yield point.

may well be complicating factors, such as stress concentration due to inhomogeneity in the material, that have not been taken into account in the above simplified treatment. The effect of such stress concentrations could be allowed for by substituting QS, where Q is a stress concentration factor, for S in equation (3); the yield point will then appear at lower values of the apparent stress S than would otherwise be the case.

ACKNOWLEDGEMENTS

It is a pleasure to thank Mr. W. H. Macey and Dr. E. Orowan for their helpful criticisms, though I do not wish to imply that either would necessarily subscribe to the views expressed in this note.

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REFLECTING-POWER MEASUREMENTS IN THE SPECTRAL REGION 2000 A. TO 1300 A.

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ABSTRACT. This report contains a description of the experimental method employed for measuring the reflection coefficients of nine materials in the spectral region 2000 A. to 1300 A., together with the results obtained.

§ 1. INTRODUCTION

The purpose of this work was to determine the reflection coefficients of the most suitable materials for the coating of mirrors and prisms, and for the production of diffraction gratings used in the region of the spectrum between λ2000 A. and λ1300 A.

Other workers have made similar measurements, but have either used oblique angles of incidence (Gleason, 1929 ; Rathenau, 1938) or have made comparison measurements against one material used as a standard (Gardiner, 1917). In the case of the lens-mirror optical systems (Johnson, 1934) and the Littrow spectrograph prisms, the angle of incidence of the light on the reflecting surface is nearly 90°, and in certain cases this is true also for the concave diffraction grating; hence it was considered desirable to measure the reflecting power for *normal* incidence at various wave-lengths in the region already stated.

§ 2. PRINCIPLE

The principle of the method will be best understood by referring to figure 1, in which a lithium-fluoride spectrograph described elsewhere (Johnson, 1938) is arranged to receive the light from the source via two routes—one, when it has been reflected from the material under test (as indicated by position 1), and secondly, when the light has passed directly to the spectrograph with the reflector removed (position 2). In the latter case the intensity of the light is controlled by placing diaphragms of various apertures at E, so that for one particular line in the two spectrograms obtained a match can be secured (see figure 2). As the amount of light transmitted by E is proportional to the area of the aperture,* the percentage reflected by the specimen may be obtained. For another wave-length some

* Remarks on the validity of this statement are given in a paper, " The Spekker Photometer for Ultra-violet Spectrophotometry ", by F. Twyman, *Trans. Opt. Soc.* **33**, 9, 1931.

Figure 2. Spectrograms taken by the method described in the text.

To face page 259

other diaphragm will be found that will give a match in the two spectra, and so on (see figure 2). Both spectra are taken on the same plate, so that conditions of development are identical. Employing such a method does not involve the necessity of knowing the characteristic curve of the photographic plate used, a factor which at present is unavailable for Ilford Q2 or Hilger-Schumann plates in the spectral region 2000 A. to 1000 A. The determination as to whether a match has or has not been obtained is decided by using a Martin densitometer (Martin, 1924). This instrument was used not for measuring the absolute density of any particular line but only for equality in density. By the arrangement of the two parallel and plane plates of lithium fluoride A and B (figure 1),

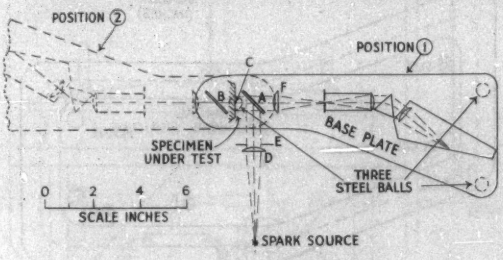

Figure 1. Base-plate for spectrograph and accessories.

equally displaced with respect to the point C, the path-length of the light (the number of reflections and transmissions by the plates) is exactly similar for both positions 1 and 2 of the apparatus.

§3. APPARATUS

This consists essentially of a vacuum chamber, the shape and dimensions of which are shown in figure 3. It is of cast iron (annealed) and has walls one inch in thickness throughout ; all the external surfaces were tinned and received several coats of a suitable cellulose paint.* The upper surface of this metal box has a flange (2″ wide) on which the lid rests. The surfaces of contact were machined, ground and scraped *after* all heating processes had been completed so as to lessen the possibility of distortion of the framework. With such precautions it was found possible to produce a fitting of the surfaces such that, by the application of Apiezon grease alone between them, a "hard" vacuum could be maintained. On the base of the container are five lugs machined with plane, slot and hole fittings so that the base-plate (shown in figure 1) can be placed into its two positions with precision. A hole H covered by a lithium fluoride

* The particular paint employed was supplied by the British Dyestuff Corporation, but others which have also proved of value in maintaining a vacuum in metal containers are "Porcenan", made by the Celstor Manufacturing Co., and "Glyptol", by the Massachusetts Institute of Technology, U.S.A.

window allows the light to enter the vacuum chamber from an external spark source operating in nitrogen. A further hole K (1" in diameter) gives exit for the pumping out of the air.

The complete casting was mounted on a suitable wooden framework so as to allow the pump to be placed immediately beneath (see figure 4). A large cone-fitting tap and a discharge tube (for judging the state of the vacuum) were arranged as shown. The pump, a "Megavac" (by the Central Scientific Supply Co., U.S.A.), reduced the pressure in the chamber to 0·01 mm. of mercury in ten

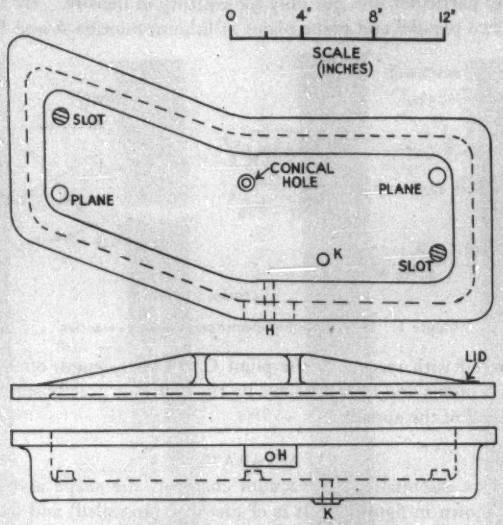

Figure 3. Vacuum chamber casting.

minutes and the discharge tube "blacked-out" (0·001 mm. or less) after twenty-five minutes. The volume of the container was 500 cu. in.

It was found more convenient to have a small crane to lift the lid (weight 75 lb.) of the casting, as it was necessary to do this twice during the operation of taking the two spectrograms. The base-plate carrying the spectrograph and accessories is shown in figure 1. On its underside three steel balls are mounted for engaging in the plane, slot and hole fittings already mentioned. The dimensions of the two parallel and plane lithium-fluoride plates A and B are $25 \times 20 \times 3$ mm. The focal lengths of the lithium-fluoride lenses D and F for $\lambda 1600$ A. are 10 cm. and 5 cm. respectively. The light from the source is rendered parallel by the lens D so that the reflections and transmissions occur in parallel light and the beam is then concentrated on the slit of the spectrograph by the lens F. The

size of the circular openings in the various diaphragms placed at E varied in diameter from 1 mm. (i.e. 1 per cent transmission) up to 3·5 mm. (12·2 per cent transmission) in stages of 0·25 mm.; from 3·5 to 6 mm. in diameter (i.e. 36 per cent

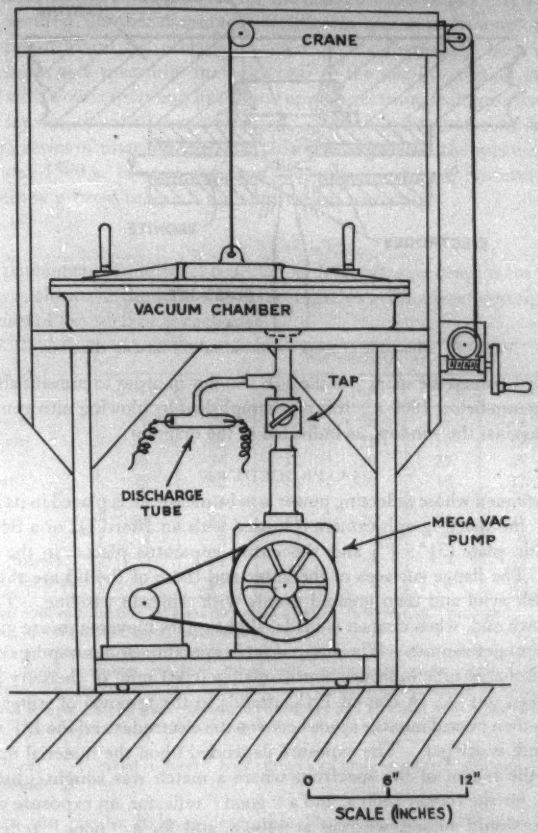

Figure 4. General view of apparatus.

transmission) in steps of 0·5 mm.; whilst 10 mm. was used as the standard full aperture.

The source is provided by a high-discharge spark between electrodes of various metals. The electrical input was approximately half a kilowatt with the

voltage stepped up by transformer to 10 000. The spark gap was 3 mm. wide. The electrodes are held in an ebonite-lined mounting attached to the side of the casing of the vacuum chamber (figure 5). It is necessary to remove the oxygen

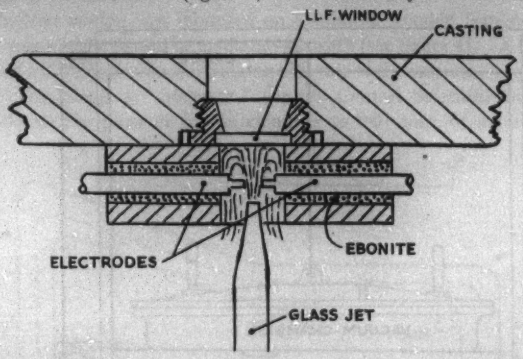

Figure 5. Mounting of spark source on side of vacuum chamber.

in the space between the spark and the LiF window in order to prevent absorption of the spectrum below 1800 A.; this is accomplished by blowing nitrogen through the spark against the window, as indicated in the diagram.

§4. PROCEDURE

The specimen whose reflecting power is to be measured is placed in its holder C (figure 1); the spectrograph camera is loaded with an Ilford Q2 or a Schumann photographic plate ($3\frac{1}{4}'' \times \frac{3}{4}''$) and the entire apparatus placed in the vacuum chamber. The flange surfaces of the latter and those of the lid are thoroughly cleaned with xylol and then greased evenly with Apiezon vaseline. The lid is lowered down and, when contact is made, slight rotary movements are given to it to ensure a proper contact. The pump is set in operation and pumping continued until the discharge tube indicates approximately 0·001 mm. of mercury pressure (i.e. " blacked out "). A tray of P_2O_5 attends to the removal of water vapour. Nitrogen is then passed into the space between the electrodes and the LiF window, and the spark is started. The exposure depended upon the material under test and upon the region of the spectrum where a match was sought; but it was found that, for the region 1800 A. and a " good " reflector, an exposure as low as one minute would suffice, whereas at 1400 A. and for a " poor " reflector ten minutes was necessary. When, however, the exposure had been made, the vacuum was released, the lid raised and the apparatus (figure 1) turned round to position 2. The surface under test is removed and the photographic plate is changed in height by an amount equal to the spectrum width. The standard 10 mm. diameter aperture-diaphragm E is then replaced by one of smaller (and estimated) area, and the lid again placed in position. The pressure is reduced

as before and an exposure of precisely the same period as that given for the first spectrogram is made. The plate is removed and developed. The two spectra thus obtained (lying side by side) are then viewed with the densitometer in order to see if a match has been obtained for the desired wave-length. If a match has *not* been secured, another diaphragm of different area must be employed for the second exposure and the process repeated. Very frequently, however, this is not necessary, as a spectrum line quite near to the one in question may be seen matched with a corresponding line in the upper spectrum, and, provided its wave-length is known, this is all that is required. Naturally it takes a considerable time to obtain a series of plates giving a match at several parts of the spectrum between 2000 A. and 1300 A. for one reflector specimen; but this, however, has been done, and the process repeated for a number of materials.

§ 5. RESULTS

The percentage of radiation reflected at different wave-lengths for the various materials is shown in the following table, together with a brief description of the preparation of the surface in each case.

Material	Wave-length (A.)						
	2026	1901	1757	1640	1570	1438	1347
Aluminium	34 %	29 %	23 %	20 %	17 %	14 %	10 %
Chromium	37	32	27	22	19	16	14
Silicon	39	34	31	29	27	25	23
Speculum	23	22	19	20	18	17	16
Silver	26	22	19	15	11	8	5
Platinum	35	33	31	30	29	28	27
Glass	3	3	5	5	7	9	8
Quartz (fused)	2	2	4	5	8	11	9
Lithium fluoride	—	—	—	2	4	5	5

Aluminium. An opaque film on glass deposited by the " evaporation " process.
Chromium. Solid piece of metal, polished.
Silicon. Solid specimen, polished with tin oxide. Surface somewhat pitted and scratched.
Speculum. Solid specimen, newly polished with magnesium oxide. Surface tarnished considerably after two months and hence reflecting power would probably decrease.
Silver. An opaque film on glass, deposited chemically and polished with rouge and cotton wool.
Platinum. An opaque film on glass, deposited by cathodic sputtering.
Glass.
Quartz (fused).
Lithium fluoride.
} Polished surfaces; back surfaces ground, to prevent reflection therefrom.

From the results it is evident that the reflection coefficients for normal incidence are distinctly low in the spectral region under consideration. It would appear that platinum has the most regular reflection throughout this region, although both silicon and chromium have a slightly higher reflecting power in the region 2000 A. The values for these two commence to drop for the shorter wave-lengths and, therefore, are not quite so suitable for some purposes. It is not without interest to note that speculum has not such a good reflecting power

as the three former materials, although it has been employed for diffraction gratings used in this region. Glass, quartz and lithium fluoride have the lowest reflecting power of those materials which were measured, but a gradual increase towards 1300 A. in the case of these three materials does appear to be real and not due to experimental error in the measurements.

§ 6. ACKNOWLEDGEMENTS

This report would be incomplete without an expression of appreciation of the work carried out by certain members of the workshop staff of the Imperial College. Mr. A. E. Davis and others were responsible for rendering the container vacuum-tight and for the accurate machining and fitting of the vacuum joints. I am indebted also to Mr. H. W. Hutchinson for his valuable help and advice in vacuum technique throughout the experiments. Without their co-operation and skill the work could not have been carried out.

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THE DETERMINATION OF $1/\lambda_2 - 1/\lambda_1$ FOR THE D_1, D_2 LINES OF SODIUM

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ABSTRACT. Newton's rings are formed between one surface of a plane parallel plate and the convex surface of a lens, and are observed in the usual manner. The incident rays are normal to the plate. The distance between the plate, which rests on the feet of an upturned spherometer, and the lens is varied and measured by the screw of the spherometer. A sodium lamp is the illuminant.

By suitable adjustment of the distance, the bright rings due to D_1-light may be made to coincide, near the centre of the ring system, either with the bright rings due to D_2-light or with the dark spaces between those rings. In the first case, a strong ring pattern is seen. In the second case, the pattern is very weak ; it is still visible, because the intensity of the D_2-light is about twice that of the D_1-light. If t be the width of the gap in either of the two cases,

$$1/\lambda_2 - 1/\lambda_1 = \tfrac{1}{4}q/t,$$

where q is zero or an even integer for a pattern of maximum strength and an odd integer for one of minimum strength. The phenomena may be traced up to $q=20$ or further. For $q=20$, t is nearly 3 mm. In one experiment, the value found for $1/\lambda_2 - 1/\lambda_1$ was 17·434 cm.$^{-1}$. The value deduced from the standard values $\lambda_1 = 5{\cdot}89593 \times 10^{-5}$, $\lambda_2 = 5{\cdot}88996 \times 10^{-5}$ cm. is 17·19 cm.$^{-1}$.

§1. INTRODUCTION

If a plate of glass *touch* the convex surface of a lens and if sodium light be used, two sets of Newton's rings, one for D_1 and one for D_2, are formed. For each wave-length there is a dark centre at the point of contact; near the centre the maxima of brightness for D_2 fall nearly on those for D_1 and the resultant ring pattern is strong.

In the case when the plate is not in contact with the lens, we consider particular values of the shortest distance—the gap—between lens and plate. To avoid complications, we suppose that the plate is " parallel " and that the incident light falls normally upon it.

Let a denote the length of the gap when the maxima for D_2 near the centre of the pattern fall on those for D_1. Then, for a given positive integer h, the gap a and a positive number m, not necessarily integral, satisfy the equations

$$2a = m\lambda_1 = (m+h)\lambda_2. \qquad \ldots\ldots(1)$$

In this case, a strong pattern appears. Because $\lambda_1 > \lambda_2$, the integer h, which may be even or odd, is positive. By (1), $1/\lambda_2 = (m+h)/2a$, $1/\lambda_1 = m/2a$, and thus

$$1/\lambda_2 - 1/\lambda_1 = h/2a. \qquad \ldots\ldots(2)$$

Let b be the gap when the maxima for D_2 near the centre fall on the minima for D_1. Then, for a given odd integer k, the gap b and a number n, not necessarily integral, satisfy

$$2b = n\lambda_1 = (n+\tfrac{1}{2}k)\lambda_2. \qquad \ldots\ldots(3)$$

The pattern is now very weak compared with that formed when D_1 and D_2 reinforce each other. It is not entirely devoid of contrast, for the intensity of the D_2-light is about twice that of the D_1-light. By (3), $1/\lambda_2 = (n+\frac{1}{2}k)/2b$, $1/\lambda_1 = n/2b$, and thus

$$1/\lambda_2 - 1/\lambda_1 = k/4b. \qquad \ldots\ldots(4)$$

If we put t for the length of the gap, there will be *either* a strong or a weak pattern when t satisfies

$$q/4t = 1/\lambda_2 - 1/\lambda_1, \qquad \ldots\ldots(5)$$

where now $q = 0, 1, 2, 3, 4, \ldots$ The weak patterns occur when q is odd.

§ 2. APPARATUS

The figures are largely diagrammatic. A spherometer S (figure 1) is mounted head downwards in the stout bar D (26 × 4 × 0·6 cm.), which is fixed to the massive tripod base W. The plane glass plate P (6 × 6 cm.) rests on the three feet of the spherometer. A wooden block (not shown) fixed to D carries four vertical strips of angle brass I (figure 2). The plate P fits, with ample clearance, into the strips, which merely prevent P from being thrown off the spherometer feet. The lens L, 1·5 cm. in diameter, is fixed by black wax to the lens bar AB, of width 1·5 cm (see figure 3); the radius of the upper surface of L is about 29·2 cm. A point C on the lens bar near A rests on the tip T of the vertical screw of the spherometer. To provide a bearing surface for the tip of the screw, a small plate of hard steel U (figure 3) is soldered to AB. A thin plate of brass V, having a hole in which the tip T fits loosely, is fixed to AB and keeps the A-end of the lens bar in approximately a constant position. The end B of the lens bar has a cross-arm fitted with two adjusting screws, X_1, X_2 (figure 2), 3·5 cm. apart, whose tips Y_1, Y_2 rest on the horizontal surface of the block M fixed to D. A safety screw X_3 (figure 2), screwed firmly into M, passes, with plenty of clearance, through a hole in the cross-arm and ensures that the ends Y_1, Y_2 of X_1, X_2 always rest on M. Two screens, shown by dotted lines in figures 1 and 2, are fixed to D; they protect the lens bar and the screws X_1, X_2 from accidental disturbance.

Light from the sodium lamp N passes through the stop K, 0·18 cm. in diameter, of the collimator J; the lens L_1 is 2·5 cm. in diameter and its focal length is 10 cm. The collimator is carried by the bar D in a manner suggested by figure 1; a joint allowing J to be tilted is convenient. Actually, the axis of the

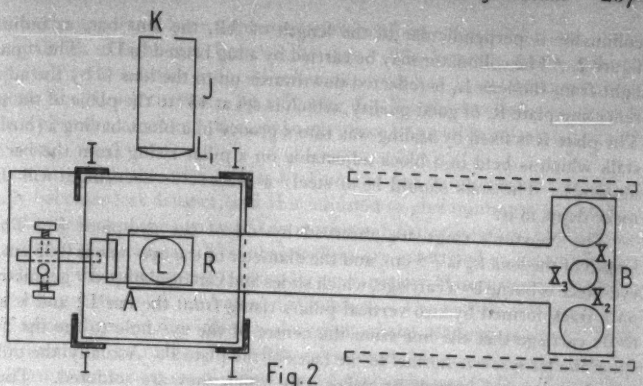

Fig. 2

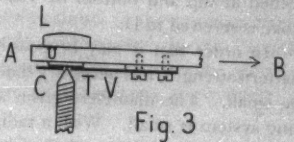

Fig. 3

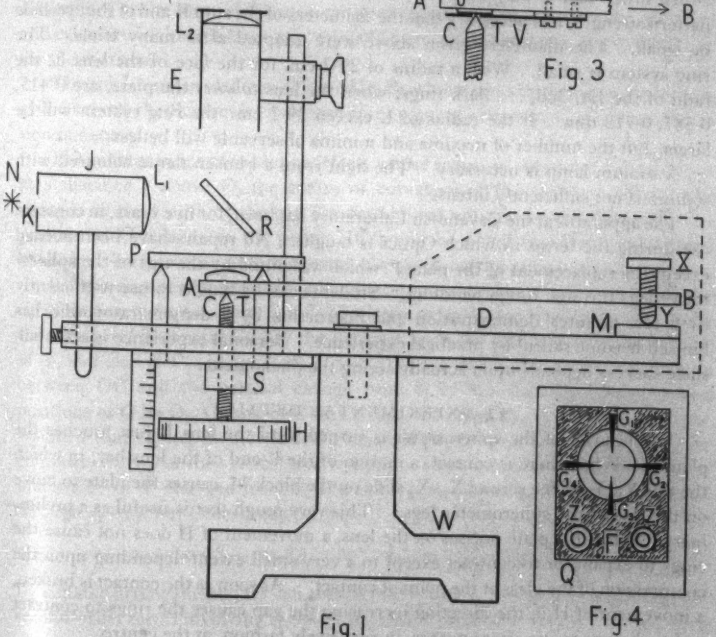

Fig. 1

Fig. 4

Figures 1 to 4. Diagrams of apparatus

collimator is perpendicular to the length of AB, the lens bar, as indicated in figure 2. The collimator may be carried by a lug brazed to D. The "parallel" light from the lens L_1 is reflected downwards on to the lens L by the adjustable reflecting plate R, of good quality, which is set at 45° to the plane of the plate P. The plate R is fixed by sealing wax into a groove in a block having a (horizontal) stalk which is held in a block adjustable on a pillar rising from the bar D (see figure 2). The stalk should be of steel; a brass screw securing it will then not make dents in it.

The Newton's rings are observed by aid of the eye-piece E. The focal length of the lens L_2 is 7·5 cm. and the diameter of the eye-hole is 0·35 cm. The eye-piece is borne by a carriage, which slides and can be clamped "geometrically" on a track formed by two vertical pillars rising from the bar D, and is so fixed to its carriage that the line from the centre of the eye-hole to the tip T of the spherometer screw is normal to the faces of the plate P. Actually, the pillars are joined at top and bottom by yokes into which they are soldered. The lower yoke is screwed to D.

In order that it may be possible to observe many maxima and minima of pattern strength, it is necessary that the diameters of the stop K and of the eye-hole be small. The diameters given above were adopted after many trials. The ring system is small. With a radius of 29·2 cm. for the face of the lens L, the radii of the 1st, 2nd, . . . dark rings, when the lens *touches* the plate, are 0·415, 0·587, 0·718 mm. If the radius of L exceed 29·2 cm., the ring system will be larger, but the number of maxima and minima observable will be less.

A sodium lamp is necessary. The light from a bunsen flame coloured with sodium is not sufficiently intense.

The apparatus at the Cavendish Laboratory has been, for five years, in constant use during the terms in which Optics is taught. No repairs have been needed except the replacement of the plate P, which was pitted by the tips of the spherometer legs through rough handling. Students can be taught to use it efficiently by a few minutes' demonstration and instruction by a demonstrator who has himself become skilful by practical experience. Personal experience is essential, since success depends upon actually seeing the phenomena.

§ 3. EXPERIMENTAL DETAILS

The head H of the spherometer is turned until the lens L just touches the plate P. When there is contact, a motion of the B-end of the lens bar, in which the tips Y_1, Y_2 of the screws X_1, X_2 slide on the block M, causes the plate to move on the tips of the spherometer legs. This very rough test is useful as a preliminary. When the plate presses on the lens, a movement of H does not cause the rings to expand or to contract except to a very small extent depending upon the compression of the glass at the point of contact. As soon as the contact is broken, a movement of H in the direction increasing the gap causes the rings to contract and to disappear one after another, in very lively fashion, at the centre.

Unless the apparatus be in adjustment, probably no rings will be seen. Two circular patches of light may, however, be seen. The larger is due to reflexion at the plate P; the smaller is due to reflexion at the lens L and will move when the screws X_1, X_2 are turned. By adjusting X_1, X_2, the smaller patch may be brought within the larger and then, if the eye-piece be suitably adjusted, the rings will be seen.

If, starting with the contact just broken, the gap be increased, the pattern gradually becomes less distinct, and H is adjusted to give minimum distinctness. A further widening of the gap causes an increase of distinctness and H is adjusted for maximum distinctness. The observations are continued as far as possible. The reading of the spherometer is taken for every pattern of maximum distinctness and for every pattern of minimum distinctness. The setting for any pattern may be made three or four times; the mean of the readings is used. If the observer examine the range of patterns a few times before taking any readings, he will learn to appreciate small differences of distinctness and will be better fitted to take accurate readings. In practice, the uncertainties in the readings for the minima are less than those for the maxima of distinctness. In the Practical Example, § 4, the mean difference, without regard to sign, between the observed and calculated values of t, the gap, is 0·00076 cm. for the minima, but 0·00113 cm. for the maxima.

The spherometer readings obtained by the above procedure do not, in general, give correctly the changes in the length of the gap. For accuracy, a slight modification is necessary.

The highest point of the lens surface, whose radius of curvature is r, is always at a distance r above O, the centre of curvature of the surface. Hence, the changes in the length of the gap are determined entirely by the changes in the position of O relative to the plate. It should be noted that the highest point of the lens surface does not, in general, always lie on the same particle of glass.

Let the distance from O to C, the point on the lens bar which rests on T, the tip of the screw, be l. Since C is fixed relative to the lens surface and so also to O, the distance l is constant. Let the distance from the under-side of the plate P of T, and also of C, change from z_1 to z_2; at the same time let the small angle between OC and the vertical change from θ_1 to θ_2. Let the corresponding positions of O be O_1, O_2 and of C be C_1, C_2; the line C_1C_2 is vertical. It is not assumed that O_2 lies in the vertical plane $O_1C_1C_2$ or that, when O_1, C_1, C_2 are collinear, O_2 lies in the vertical line $O_1C_1C_2$. Then the distance of O below the under-side of P changes from $z_1 + l\cos\theta_1$ to $z_2 + l\cos\theta_2$. Thus T descends through $z_2 - z_1$, but O descends through

$$z_2 - z_1 + l(\cos\theta_2 - \cos\theta_1) \quad \text{or} \quad z_2 - z_1 - 2l(\sin^2\tfrac{1}{2}\theta_2 - \sin^2\tfrac{1}{2}\theta_1).$$

By suitable arrangements, we can secure that θ_1 and θ_2 are both so small that the second-order terms involving them are negligible compared with $z_2 - z_1$, and then

the changes in the length of the gap may be deduced from the spherometer readings without correction.

The lens bar has to be adjusted so that OC is vertical. The adjustment is made by aid of a carriage Q (figure 4) bearing a frame F, in which are set four needle points G_1, G_2, G_3, G_4. The carriage is geometrically located on supports on the bar D. When the plate P is in place, it is a little below the frame and its carriage. With the lens bar and the plate P removed, the screw is so adjusted that its tip T is at the level of the needles. The frame F is then so adjusted relative to Q that G, the point of intersection of the lines G_1G_3 and G_2G_4, coincides with T. The carriage is then locked in position by the screws Z, Z. The setting of F is a "maker's adjustment", and, once done, need not be repeated. The carriage, with the frame, is then removed, the lens bar and plate are put into position and the carriage is replaced in its geometrical seating. The reflector R is put in place and the lamp is started. The screw is adjusted so that the lens is just out of contact with the plate, and the screws X_1, X_2 are adjusted so that the centre of the ring pattern, as viewed through the eye-piece, is covered by the point G. If the eye-piece be set so that the rings are seen most distinctly, the needles will be a little out of focus, but the indication they afford suffices. As already explained, the centre of the eye-hole is vertically above the tip T of the screw.

The line OC has now been made very nearly vertical; during the measurements it is kept very nearly vertical by aid of the screws X_1, X_2. As the gap is increased, the ring centre moves towards the B-end of the lens bar. The ring centre can be brought back to the point G by X_1, X_2. This adjustment is made whenever the centre has moved more than one or, at most, two mm. from G.

The readings can now be taken. The reading for contact is first obtained. Then the gap is increased until the first minimum distinctness of pattern is observed, and the reading of the screw head is taken. The screw is then turned until maximum distinctness is reached. The values of q for these three settings are 0, 1, 2. The process may be continued until q reaches 20 or more, but the observations begin to be difficult when q exceeds 10 or 12. A good deal depends on the acuteness of vision and the keenness of judgment of the observer.

If $2n+1$ patterns have been observed, so that q goes from 0 to $2n$, we may subtract the readings for $q=0, 1, 2, \ldots n$ from those for $q=n, n+1, n+2, \ldots 2n$. We obtain the $n+1$ differences $d_0, d_1, \ldots d_n$. Let their sum be Σd. The mean $\Sigma d/(n+1)$ gives an average value of t for $q=n$. Then

$$\frac{1}{\lambda_2}-\frac{1}{\lambda_1}=\frac{1}{4}\frac{n}{\Sigma d/(n+1)}=\frac{n(n+1)}{4\Sigma d}. \qquad \ldots\ldots(6)$$

This is the quickest method of obtaining a result, but the method of Awbery (§ 4) is better, for it enables us, in principle, to compare the observed values of t with those given by the "best" straight line through the plotted points. With careful observations the points lie so near to a straight line that an arithmetical, as opposed to a graphical, method must be used, if the full accuracy obtainable is to be attained.

§ 4. PRACTICAL EXAMPLE

The pitch of the screw is 0·05 cm. The head has 25 numbered divisions, each corresponding to 0·002 cm. Each division is subdivided to tenths. The table gives the spherometer readings and the corresponding values of the "observed" length t of the gap. The values of t "calculated" are those found below by Awbery's method. The patterns of minimum distinctness are marked *. (T = turns, H = head reading.)

q	T	H	t (obs.)	t (calc.)	q	T	H	t (obs.)	t (calc.)
		(div.)	(cm.)	(cm.)			(div.)	(cm.)	(cm.)
0	0	17·3	0·0000	−0·00036	8	2	23·5	0·1124	0·11435
1*	0	24·5	0·0144	0·01398	9*	3	7·6	0·1306	0·12869
2	1	5·2	0·0258	0·02832	10	3	14·3	0·1440	0·14303
3*	1	13·8	0·0430	0·04266	11*	3	21·3	0·1580	0·15737
4	1	20·8	0·0570	0·05699	12	4	2·6	0·1706	0·17171
5*	2	3·4	0·0722	0·07133	13*	4	10·8	0·1870	0·18605
6	2	10·5	0·0864	0·08567	14	4	16·8	0·1990	0·20039
7*	2	17·2	0·0998	0·10001					
28			0·3986		77			1·1016	

When t is plotted against q, the points lie nearly on a straight line. We use Awbery's method (Searle, *Experimental Physics*, p. 360) to find the "best" straight line. If e, f be the q and t coordinates of the centroid of the first group, of 8 points,

$$e = 28/8 = 3{\cdot}5, \qquad f = 0{\cdot}3986/8 = 0{\cdot}049825.$$

If h, k be the centroid of the second group, of 7 points,

$$h = 77/7 = 11, \qquad k = 1{\cdot}1016/7 = 0{\cdot}157371.$$

If μ be the gradient of the straight line through the two centroids,

$$\mu = (k-f)/(h-e) = 0{\cdot}107546/7{\cdot}5 = 0{\cdot}0143395.$$

The "best" value of $1/\lambda_2 - 1/\lambda_1$ is derived from this gradient, and, by (5),

$$\frac{1}{\lambda_2} - \frac{1}{\lambda_1} = \frac{1}{4 \times 0{\cdot}0143395} = 17{\cdot}434 \text{ cm.}^{-1}.$$

The equation to the straight line through the two centroids is

$$t = f - \mu e + \mu q = -0{\cdot}0003632 + 0{\cdot}0143395q, \qquad \ldots\ldots(7)$$

for this line passes through $q = e$, $t = f$, and has the gradient μ. The values of "t (calc.)" derived from (7) for $q = 0, 1, 2, \ldots$ are given in the table.

With $d_0 = t_7 - t_0$, $d_1 = t_8 - t_1, \ldots$ we find $\Sigma d/(n+1) = 0{\cdot}8028/8$. Then, by (6),

$$1/\lambda_2 - 1/\lambda_1 = 7 \times 8/(4 \times 0{\cdot}8028) = 17{\cdot}439 \text{ cm.}^{-1}.$$

The standard values of λ_1 and λ_2 are

$$\lambda_1 = 5{\cdot}89593 \times 10^{-5}, \qquad \lambda_2 = 5{\cdot}88996 \times 10^{-5} \text{ cm.}$$

Hence $1/\lambda_1 = 16960{\cdot}85$, $\qquad 1/\lambda_2 = 16978{\cdot}04$ cm.$^{-1}$,

and $$1/\lambda_2 - 1/\lambda_1 = 17{\cdot}19 \text{ cm.}^{-1}.$$

COLOUR, ITS MEASUREMENT, DISCRIMINATION AND SPECIFICATION *

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ABSTRACT. The principles of colour specification and measurement are analysed and considered from the point of view of colour tolerances. Colour tolerances are considered under three main headings: (α) liminal, on basis of R.U.C.S., (β) technical, in terms of phenomena possibly involved, and (γ) economic. Tolerances for subtractive colour mixtures are analysed for practical application. A uniform-chromaticity scale of saturation is proposed.

§ 1. INTRODUCTION

COLOUR, in a special sense of the word, is a measurable quantity and therefore is subject to tolerances which are an essential part of a colour specification. The specification and control of colour in metrical terms, however, bring colour measurement into relation with the concrete, everyday applications of colorimetry and raise questions of interpretation in terms of conceptions of colour which, although having some physical basis, differ from that which affords a true foundation for colour measurement. It will be desirable first to consider these briefly in order to free them from some current misconceptions.

No definition of colour, to be precise and useful in the investigation of any of the many phases of the subject, could be so comprehensive as to be equally applicable in all of these. We are not, for example, directly concerned here with the psychological reactions which colour may produce nor with the various intermediate transformations undergone by the energy in the stimulus while on the way to its ultimate manifestations in consciousness. The connotations of suitable working definitions within this field must therefore necessarily be based upon an analysis of the whole process and each be limited to a particular coherent *ensemble* of characteristics selected from those commonly and generally associated with "colour". Bearing in mind the richness of the whole subject, there is therefore, clearly, a real need for the introduction of new and appropriate terms of limited connotation to denote the many differing phenomena and concepts all currently comprehended within the broader meaning of that ambiguous word. This, however, involves a study reaching far beyond the scope of the present analysis, in which we confine attention to colour as it is perceived under normal

* A paper read at the first meeting of the Physical Society Colour Group.

conditions and restrict consideration to the phenomenon in consciousness conceived as in direct causal relationship with the exciting physical stimulus.

The definitions of colour which need to be taken account of in dealing with the various possible modes of expression and application of colour specifications and tolerances assume three basic forms subject to certain subsidiary conditions. These forms differ essentially in the direction of their emphasis, which falls respectively on the psychological, psycho-physiological and physical phenomena involved, and are as follows:

I. *Colour is the mode of consciousness resulting from the action of a train of light waves upon the receptive organs of vision*

Experiment shows that it is only necessary here to take account of amplitude and frequency when specifying the physical stimulus, which is therefore sufficiently defined by the usual expression of the results of spectrophotometric analysis given in the form $I=f(\lambda)$. Such analysis of the light does not, of course, provide a basis for the measurement of the colour (except in respect of one of its essential variables), although colour can be identified unambiguously in this way. It is frequently desirable in practice to be able to relate colour and colour tolerances to this form, because the determination of colour differences as such provides, so to speak, merely a synoptic view of the corresponding spectrophotometric variation which may be very complex and which alone provides the key to changes in composition.

An alternative basis for definition is provided by the subjective analysis of colour into the attributes: hue, purity and brightness. These qualities can be correlated in serial order, by virtue of the psycho-physical nexus provided by vision, with the physical quantities: (i) wave-length (λ), (ii) relative admixture (μ) of white light, and (iii) intensity (w) relative to some standard; the whole complex which may be termed a "sensation-correlate", characterizing an additive mixture, of monochromatic and white lights, visually indistinguishable from a given mixed radiation, of colour to be specified. If we accept this correlation as unique and adequate, except for the class of "purples" definable by their complementaries, colour may be defined as follows:

II. *Colour is the physical sensation-correlate* (λ, μ, w)

This definition has some uses and is convenient as a practical device for interpreting colour variations in terms of quantities recognizable by the large majority of those interested in the broader aspects of colour, distinct from colour measurement. It is fundamentally unsuitable as a basis of colour measurement, however, as one of its defining data (λ) lacks a quality essential to this purpose. This is clear from the fact that the quantities λ are essentially heterogeneous and of unlike kind in the property involved, and so do not admit the relation of greater or less in this component variation of the quantity, colour, to be measured. This definition is unsuited to physical purposes also for other reasons (Guild, 1925).

The third basis for definition satisfies the conditions for measurement and is afforded by the trichromatic, more correctly "tristimulus", analysis of colour in terms of three fixed unitary stimuli. By the C.I.E. (1931) system (Smith and Guild, 1931; Judd, 1933) X, Y, Z represent three such units now widely adopted, in terms of which the "colour quality" or "chromaticity" may be expressed quantitatively in the form

$$xX+yY+zZ,$$

where $x+y+z=1$. This, in association with the brightness factor, w, provides the basis for colour measurement in the now familiar form. To this complex corresponds a definition of colour (Form III), but whether in this case colour be defined as a purely physical quantity or as a physical sense-correlate depends upon creed and point of view and will be briefly discussed in a later section. The corresponding form of statement of any colour is:

III. $(xX+yY)$, w,

the inclusion of the z variable being, of course, unnecessary.

Brightness, field size and other physical conditions, including ultimately also those affecting the observer, are assumed to be included in such definitions. A general formulation of colour would relate it functionally to all such conditions, but there would still be a practical need to consider this abstract but simple and physically elemental form.

§ 2. COLOUR MEASUREMENT

Metrical tolerances are, generally speaking, the permissible limits of departure in the magnitude of a measurable quantity from a given magnitude known to be desired. To the extent that such limits are dependent upon the limitations that may apply only in some particular instance, their assessment involves considerations of a specialized character. Colour, however, provides a basis for a formulation of colour tolerances also in a general sense related to the limitations of perception and, correspondingly in this case, of measurement. In the simple limiting case the tolerance is dependent solely upon the discrimination limen, and this case will be considered first.

All processes of measurement providing data for a physical analysis of the world depend ultimately upon immediate judgements of equality or equivalence and so are subject to their inevitable latitude and dispersion about a mean point or interval. In applying processes of physical investigation to the phenomena of colour with the object of reducing certain aspects thereof to a quantitative basis, the immediate frontier of the external world is pressed back indefinitely into the region of the phenomena of perception, observed uniformities in which are formulated in terms of physical units, without regard, of course, to the subjective estimation of quantity. (This latter, although capable of providing further data for the analysis of processes of perception, naturally plays no part in investigations which are to be verifiable by any normal observer.) Such a

procedure adequately illuminates the physical side of the picture and is clearly legitimate.

In some respects important to the present subjects, however, the processes involved in the measurement of colour differ fundamentally from those by which measurement is made upon most other quantities. Generally speaking, in physical measurement, judgements of equality are applied *indirectly* to the quantity being measured and a quantity of variable magnitude. Measurement in such cases can therefore be refined by relating the threshold of perception to a more highly subdivided scale of magnitude in the units in question, thus rendering less significant the variations due to such causes. On each such successive application, at least up to a certain stage, this procedure generally affords support and justification for the possibly superficial but at any rate convenient and fruitful working assumption of continuity in the quantity to be measured.

In colour measurement, however, the criterion of the equality judgement is applied *directly*, certainly in the establishment of standards, and also, it may be, in the actual measures obtained, so that no such ultimate refinement in measurement as was alluded to above is here possible. Such measurements depend upon individual settings which, on account of the finiteness and variability of the discrimination limen, would not be distributed strictly symmetrically and " normally " in the Gaussian sense ; but we may assume that a suitable reduction of observations is made. Nevertheless, one should in this case distinguish between a measure of the physical stimulus and the actual quantity being measured, which here retains within itself some subjective elements and is capable of being discriminated only in steps which are essentially finite. The quantity to be measured, if considered limited in essence to characteristics known from sense data, must therefore be considered as inherently indefinite in magnitude.

This may be considered too conservative a view to take, and out of accord with current physical conceptions, and so, in order to provide a means of escape, one may prefer to exceed the limitations of sense perception and to hypostatise a more acceptable definiendum. The term *colour* might then legitimately denote a continuously variable entity and be regarded as accurately representing the selected section of the phenomenon in question, but this interpretation would be justifiable only by virtue of an assumption which would of necessity be arbitrary. As an alternative, continuity might be ascribed to the quantity merely as a convenient fiction.

Questions of this nature involving the degree of indefiniteness properly characterizing the term colour and its appropriate definition naturally become more acute on the resort to objective methods of colour measurement. In the application of such methods, measurement is made primarily of a conventional function of the stimulus, and it is assumed that this accurately represents the phenomenon of colour: but, assuming that the conventional basic data permit an indefinitely high accuracy in objective measurement, then however accurately such a measurement may be made in terms of physical units, and however it may

transcend the limits of discrimination under certain conditions of visual observation or measurement, the ultimate purpose is still the control of perceived colour to an accuracy rendered necessary by the higher powers of discrimination under some other attainable conditions. Here again, therefore, the live entity in question can only be known in finite steps; and such steps, and the conditions under which they apply, therefore form an essential part of the definition.

That this must be so, except possibly in abstract reasoning, may be seen to be the case on logical as well as physical grounds. For (*a*) the definition is required to be existential and not merely conventional. (*b*) It is required to define a quantity from which the subjective element can be excluded only by convention. Hence the conditions of existence of such a quantity involve the limits of colorimetric equality, which are therefore implied by the definition. It follows that any such term whose definition were not so qualified would not comply with one of the Laws of Thought (A is not not –A) and would be inadmissible. Such a term if admitted would in use be a fertile source of fallacies.

It has long been recognized that conditions of observation should be part of the definition of colour. The limits of colorimetric equality are thus also an essential part.

For the reasons here set forth, the above basic forms of definition may need qualification and two alternatives present themselves, viz :

Case a. " Colour " may connote a continuously variable quantity having a distinct mode for every different value of each of its defining variables and pertaining to an entity existing by supposition or as ideal beyond the limitations of colour discrimination.

Case b. " Colour " may connote a continuously variable quantity having, for every one of its possible modes under given conditions of observation, a range of colorimetrically equivalent, composite values formed by variables the numerical values of which are comprised within certain stated, mutually dependent intervals.

It will only be necessary to consider these cases directly in their application to Form III, the resulting modified forms being respectively as follows:

III *a*. The resulting colorimetric formulation in terms of brightness factor and the unitary stimuli X, Y, of the C.I.E. system, is as given in III, viz.

$$(xX+yY),\ w.$$

III *b*. In this case certain intervals are prescribed by the definition which are related to the colour discrimination limens under the stated conditions of observations. Denote these limits by sets of values ξ_j, η_j, ϖ_j corresponding to and in the same units as x, y, w respectively. The alternative formulation in terms of the same units will then be as follows:

$$(x \pm \xi)X + (y \pm \eta)Y,\ w \pm \varpi.$$

where ξ, η and ϖ may have any values between the limits given respectively by

$$\xi^2 \leqslant \xi_j^2,\ \eta^2 \leqslant \eta_j^2;\ \text{for } \xi/\eta = \xi_j/\eta_j$$

and

$$\varpi^2 \leqslant \varpi_j^2.$$

It is assumed for convenience in practice that the absolute value of the limen is unaffected by a change in sign. This is a sufficient approximation, at least for a normal observer. The nature of possible dependence of ϖ_j upon values of ξ_j and η_j cannot at the present time be clearly stated.

On considering these definitions in the light of the foregoing discussion, one may note that in case (*a*) there is no question of correlating " defined colour " with perceived colour. The quantity is considered susceptible of unlimited subdivision and discrimination between its values. It is not known at what particular stage in the process of perception the colour-discrimination threshold appears, although hypotheses have been put forward to explain this quantitatively by abstract relationships (Helmholtz, 1896 ; Houston, 1922; Smith, 1932; Judd, 1935). If this limitation be found to be explicable on physical grounds, such a form would be ultimately untenable, but it would still have theoretical value as a limit.

In Case (*b*) " defined colour " is correlated with perceived colour according as this is found to be differentiable under specific conditions of observation ; but naturally only at the expense of generality, since the definition is necessarily limited in application to such conditions. To each set of such conditions corresponds a different family of values ξ_j, η_j, ϖ_j and hence a different definition. A unique definition is furnished only by III *a*, which, however, is adequate to meet only idealized conditions.

Thus III *a* and III *b* fulfil different rôles, that of III *a* being limited to abstract analysis, while that of III *b* is appropriate to the specification of a colour in which definite conditions of observation are assumed.

The application of III *b* in practice requires a general adoption of data specifying the colour discrimination limen completely for all points of the colour field. Information on this subject is accumulating at the present time, and it is to be hoped that the near future will bring a freely expressed international acceptance of standard data for this property of colour vision. The use of available approximate data as a basis for the assessment of colour tolerances will be considered below.

§ 2.1. *Colour tolerances from the point of view of discrimination and measurement*

In the limiting case, when the various conditions and requirements in the practical realization of a colour tend conjointly to enable limits to be imposed which are small by comparison with the colour-discrimination limens, no further problem presents itself in the specification of a colour beyond ensuring that these limits are known in a convenient form. This would in practice be a rare case, however, for generally the practicable limit will be at least comparable with the discrimination limen. The limen applying under some standard conditions may then serve as a unit of colour variation by which (*a*) to measure the extent of the variations corresponding to other conditions and (*b*) to set a limit

independent of these. The need for a convenient formulation of colour-discrimination limens will first be treated.

On the rectangular colour chart of x, y in C.I.E. units, the values ξ_j, η_j for a normal observer under standard conditions and for some given point (x, y) represent very approximately an ellipse. This ellipse may vary in dimensions and orientation over the whole colour chart and requires to be defined numerically for every point x, y. The values ξ_j, η_j are known, of course, at present not by deductions from theory, but by interpretation of observational data obtained (Martin *et al.*, 1940) in specific cases. It may be remarked here in passing that the determination of the general colour-limen depends upon observations which involve directly the differentiation of sensations, and are thus of a type generally excluded from physical investigations. Here and in the investigation of the processes of colour perception they are nevertheless of primary importance.

Isolated sets of observations of ξ_j, η_j are apt to be very misleading, as was found by Judd (1932 and 1935), who has made a critical examination of the available mass of data and adapted to it a transformation of the Maxwell triangle, as based on the C.I.E. (1931) recommendations, with the object of satisfying a condition which would, in effect, relieve workers of the need for ascertaining the constants for the liminal ellipse for any given point (x, y). This aimed at such a transformation of the Maxwell areal coordinates that the ellipses would be transformed into circles of equal radius. The system put forward by Judd was modified by MacAdam (1937) and by Breckenridge and Schaub (1939), the latter authors presenting, with a balanced exposition, a "rectangular uniform chromaticity system" (R.U.C.S.) which secures an advantageous form for the colour diagram in addition to at least approximately uniform chromaticity, and thereby meets general requirements to such an extent that the system is probably, for practical use, the most generally acceptable one of this type that is possible. The use of this system in practice is considered by Holmes (1940). It has the incidental disadvantage of being based upon three unitary stimuli all with non-zero luminosity coefficients. The system may have some value as a secondary transformation. From the point of view of primary standardization, the cost to the indefinite future of the balance between advantages and disadvantages of a change in standards should not be overlooked when considering the effect upon the colour records merely of a decade.

No one system can have all the advantages, however, and the immediate reason for considering the R.U.C.S. consists in the fact that it affords a general working basis for the practical formulation of a two-dimensional chromaticity scale.

§ 2.11. *Liminal colour tolerances in R.U.C.S.*

The accumulation of further observational data on the chromaticity limens may indicate that the chromaticity scale of the R.U.C.S. is not ultimately accurate, but it does not seem probable that it would be seriously in error or that any other type of system would combine to a comparable degree an equal number of advantages. Ultimate accuracy of liminal data is, moreover, not essential in the

assessment of colour tolerances. The R.U.C.S. is therefore adopted in the ensuing sections as a secondary system providing a chromaticity scale which is considered sufficiently uniform for practical purposes, so that colour tolerances having a purely liminal basis, here referred to as liminal tolerances, may be deduced by its aid.

In terms of the units of the R.U.C.S. system, and using the variables u and v to replace the x'' and y'' of Breckenridge and Schaub, let the corresponding chromaticity limens be ξ'' and η'' respectively. Then a colour tolerance for the point (u, v), based solely upon the chromaticity limen for this point, would be given in the convenient form

$$\xi''^2+\eta''^2=\rho_1''^2,$$

where ρ_1'' is some constant.

Likewise let the colour tolerance affecting the brightness factor be given by the quantity ϖ_1. This will, generally speaking, be based upon the application of the Fechner fraction to the brightness factor w.

Chromaticity and brightness are, it is true, analytical properties, but sensation itself in this case concurs in this physical analysis. One might therefore suppose that the eye would be capable of perceiving colour differences only when these are discernible separately as differences in chromaticity and/or brightness. But in practice this tends to result in an unduly large limen for some composite variations. The inference seems justified that colour differences may be perceptible when, if analysed in terms of chromaticity and brightness, the component differences are individually smaller than the corresponding limens under constant conditions. The composite quantity representing colour must therefore continue to be regarded as a corporate entity in the application of colour tolerances.

Information on the perception of combined variations of brightness and chromaticity is very meagre. The situation has been considered by Balinkin (1939) and a definite formulation put forward. This is adopted here with slight modification as follows:—If for the point (u, v) under given conditions the chromaticity limen, or a multiple of it, suitable for use as chromaticity tolerance in some given instance, be ρ_0'' in R.U.C.S. units, and if the brightness limen or some suitable multiple thereof be ϖ_0, then the composite liminal tolerance is represented by all sets of values ξ_j'', η_j'', ϖ_j satisfying the equation

$$\frac{\xi''^2+\eta''^2}{\rho_0''^2}+\frac{\varpi^2}{\varpi_0^2}=1.$$

Limiting values for ξ, η and ϖ are thereby all determined in a manner convenient for immediate application, and represent in rectangular coordinates an ellipsoid forming the boundary to points falling within the tolerance The parametric form is preferable for practical use, viz.

$$\xi_j=\rho_0''\cos\theta_j\sin\phi_j,$$
$$\eta_j=\rho_0''\sin\theta_j\sin\phi_j,$$
$$\varpi_j=\varpi_0\cos\phi_j.$$

Any adjacent point would then have the coordinates θ_j, ϕ_j, which may readily be found and used to check the variation against the tolerance.

§ 2.12. *Liminal tolerances in C.I.E. system*

The same principles may be applied to the deduction of liminal tolerance in the variables (x, y, w). In this case the chromaticity variation comprised in the tolerance will be deduced from the corresponding variation in the coordinates of the R.U.C.S. The variations ξ_j and η_j respectively in x and y, corresponding respectively to the limiting variations ξ_j'' and η_j'' in u and v defined in the preceding section, deduced as small variations by the R.U.C.S. transformation, are as follows:

$$\xi_j = A\xi_j'' + B'\eta_j'',$$
$$\eta_j = A'\xi_j'' + B\eta_j'',$$

where

$$A = +q_2/(p_1q_2 - p_2q_1),$$
$$A' = -q_1/(p_1q_2 - p_2q_1),$$
$$B = +p_1/(p_1q_2 - p_2q_1),$$
$$B' = -p_2/(p_1q_2 - p_2q_1),$$

in which

$$p_1 = (-\ 6{\cdot}6282\ y - 0{\cdot}52693)/(x - 7{\cdot}0534\ y - 1{\cdot}64023)^2,$$
$$p_2 = (+\ 6{\cdot}6282\ x - 7{\cdot}1551)/(x - 7{\cdot}0534\ y - 1{\cdot}64023)^2,$$
$$q_1 = (-20{\cdot}9991\ y - 4{\cdot}6950)/(x - 7{\cdot}0534\ y - 1{\cdot}64023)^2,$$
$$q_2 = (+20{\cdot}9991\ x - 1{\cdot}32811)/(x - 7{\cdot}0534\ y - 1{\cdot}64023)^2.$$

Conditions in practice do not always render it convenient to carry through calculations of this type to ascertain the limiting values of the tolerance, under certain prescribed conditions. For this reason, and since the accuracy here required is not high, use may very conveniently be made of tabulated data or of colour charts (Holmes, 1940; Hilger, 1941), giving at a glance the relation of the C.I.E. scale to the colour scale upon which liminal tolerances are based.

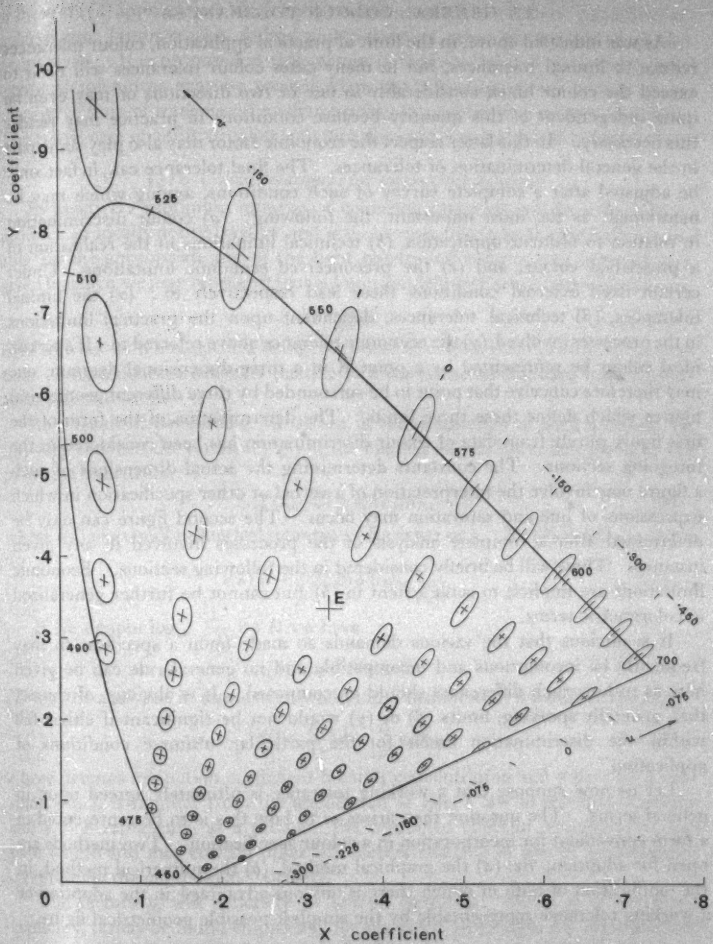
It may be seen from the figure, which is a graphical representation on the rectangular x, y colour-chart of one arbitrary R.U.C.S. chromaticity step about points at equal intervals on the u, v scale, that the R.U.C. scale undergoes considerable distortion by the transformation to the C.I.E. scale. In this diagram the loci are plotted about the x, y points corresponding to the u, v points given by

$$u = \pm 0{\cdot}0375\nu_1,$$
$$v = \pm 0{\cdot}0750\nu_2,$$

ν_1 and ν_2 being positive integers, and the chromaticity variation corresponds to $\rho_0'' = 0{\cdot}01$.

The complete liminal tolerance in C.I.E. units will be similar in general form to that applying in the preceding section and is given in the following:

$$\xi_j = (A\xi_j'' + B'\eta_j'') \sin \phi_j,$$
$$\eta_j = (A'\xi_j'' + B\eta_j'') \sin \phi_j,$$
$$w_j = w_0 \cos \phi_j.$$



C.I.E. colour chart, showing relative magnitude on C.I.E. scale of a constant small uniform chromaticity step (R.U.C.S. scale) for points ($u = \pm 0375\ v_1$; $v = \pm 075\ v_2$).

§ 3. GENERAL COLOUR TOLERANCES

As was indicated above, in the limit of practical application, colour tolerances reduce to liminal tolerances, but in many cases colour tolerances will need to exceed the colour limen considerably in one or two directions or may even be quite independent of this quantity because conditions in practice may render this necessary. In this latter respect the economic factor may also play some part in the general determination of tolerances. The final tolerance can, in fact, only be adjusted after a complete survey of such conditions, among which may be mentioned, as the most important, the following: (*a*) colour discrimination in relation to ultimate application, (*b*) technical limitations in the realization of a prescribed colour, and (*c*) the preconceived economic limitations. Under certain fixed external conditions these lead respectively to : (α) the liminal tolerances, (β) technical tolerances, dependent upon the practical limitations in the processes involved, (γ) the economic tolerance above referred to. If a certain ideal colour be represented by a point R in a three-dimensional diagram, one may therefore conceive that point to be surrounded by three different geometrical figures which define these three limits. The determination of the form of the first figure purely from data of colour discrimination has been considered in the foregoing sections. The constants determining the actual dimensions of such a figure may involve the interpretation of a verbal or other specification in which expressions of hue and saturation may occur. The second figure can only be determined after a complete analysis of the processes involved in any given instance. These will be briefly considered in the following sections. Economic limitations are implicit to some extent in (*b*), but cannot be further generalized in colorimetric terms.

It is obvious that the various demands so made upon a specification may frequently be incongruous and incompatible, and no general rule can be given here as to how such differences should be composed. It is obvious, of course, that, generally speaking, limits (β) or (γ) would not be significant if either fell within the discrimination limen for the particular ultimate conditions of application.

Let us now suppose that a working tolerance is ultimately agreed upon in general terms. The question then arises as to how this is to be represented in a form convenient for incorporation in a colour specification. Two methods are open for adoption, viz. (*a*) the graphical method, (*b*) the numerical method, in the application of both of which there is obvious advantage in the adoption of a working tolerance representable by the simplest possible geometrical figure.

§ 3.1. *Technical colour tolerances*

In the preceding section, reference was made to the need for considering technical limitations when framing any colour specification. This is clearly a very wide subject, as it must comprehend all physical causes upon which the ultimate production or modification of colour may depend. The commonest

of these are (*a*) approximate black-body emission and (*b*) absorption; but diffusion, polarization, luminescence, fading and numerous other phenomena may also be involved. It is proposed in this section to indicate, by way of example, how technical tolerances may be treated when the colouring means consist of an absorptive train of transparent media resulting in a subtractive mixture of colour. The trivial case of additive colour mixture does not require special consideration.

§3.11. *Colour tolerances for subtractive mixtures*

In practice a minimum number of colouring materials will be employed to produce an ideally desired colour, and if we assume this to be theoretically attainable by the appropriate use of the light source and components assumed in a given instance, we may represent that colour uniquely by Form I (*supra*) as follows:

$$I = f(\lambda), \qquad \ldots\ldots(1)$$

light sources with line spectra being, of course, excluded from present consideration. I here denotes intensity relative to a conventional standard having equi-energy distribution.

As a result of permissible variations in the production of such a colour, let $f(\lambda)$ vary between the limits of $f'(\lambda)$ in the manner and to the extent given by the following expression:

$$f'(\lambda) = \{1 \pm \epsilon(\lambda)\} f(\lambda), \qquad \ldots\ldots(2)$$

where $\epsilon(\lambda)$ is a small numerical quantity, a function of λ. By (2) it follows that

$$\epsilon(\lambda) = -2{\cdot}3026\,\delta \log_{10} \frac{1}{f(\lambda)}. \qquad \ldots\ldots(3)$$

If we denote $\log_{10} \dfrac{1}{f(\lambda)}$ by D we have

$$\epsilon(\lambda) = -2{\cdot}3026\,\delta D.$$

δD may be analysed in terms of the individual variations of k media in the form

$$\delta D = \sum_{i=1}^{k} \delta d_i = \sum_{i=1}^{k} e_i \delta p_i; \qquad \ldots\ldots(4)$$

e here denotes extinction coefficient for unit concentration and p denotes a constant which is familiar from the application of Beer's law to transparent media and which represents, as a product, the combined effect of concentration and path length and may contain other like factors entering in special applications; d represents the component "density".

The factors p_i will generally be known forthwith. When p_i is not given explicitly, it may be found empirically for any given component by measurement to ascertain the effect upon D of a variation, $\theta_i p_i$, in p_i, caused solely by a fractional variation, θ_i, of any one factor of p_i which can conveniently be varied in practice. The value of p_i may then be found by substitution into

$$p_i = \delta D / \theta_i e_i, \qquad \ldots\ldots(5)$$

which is derived from (4).

$\epsilon(\lambda)$ will now assume the form

$$\epsilon(\lambda) = -2{\cdot}3026 \sum_{i=1}^{k} \theta_i p_i e_i = -2{\cdot}3026 \sum_{i=1}^{k} \theta_i d_i, \quad \ldots\ldots(6)$$

where θ_i receives values later to be evaluated for each component.

For the ideal colour we have as the prescribed chromaticity in C.I.E. units,

$$x, y, z = \frac{\int_V fE(\bar{x}, \bar{y}, \bar{z})d\lambda}{\int_V fE(\bar{x}+\bar{y}+\bar{z})d\lambda} \quad \ldots\ldots(7)$$

and the brightness factor

$$w = \frac{\int_V fE\bar{y}\, d\lambda}{\int_V E\bar{y}\, d\lambda}. \quad \ldots\ldots(8)$$

The continuous functions implied by the C.I.E. (1931) tables are identified with the corresponding variables E, $\bar{x}$, $\bar{y}$, $\bar{z}$ here employed. V denotes the range of the visible spectrum.

The variations in the quantities x, y, z and w, represented respectively by ξ, η, ζ and ϖ, due to the above contemplated variation in $f(\lambda)$, may be deduced from these equations and expressed in the following form:

$$\left.\begin{aligned} \xi &= \{a(B+C) - A(b+c)\}(y/w\mathscr{Y})^2, \\ \eta &= \{b(C+A) - B(c+a)\}(y/w\mathscr{Y})^2, \\ \zeta &= \{c(A+B) - C(a+b)\}(y/w\mathscr{Y})^2, \\ \varpi &= (b/\mathscr{Y})^2, \end{aligned}\right\} \quad \ldots\ldots(9)$$

where

$$\left.\begin{aligned} A &= \int_V fE\bar{x}\, d\lambda, \quad a = \int_V \epsilon fE\bar{x} d\lambda, \\ B &= \int_V fE\bar{y}\, d\lambda, \quad b = \int_V \epsilon fE\bar{y}\, d\lambda, \\ C &= \int_V fE\bar{z}\, d\lambda, \quad c = \int_V \epsilon fE\bar{z}\, d\lambda, \\ \mathscr{Y} &= \int_V E\bar{y}\, d\lambda, \end{aligned}\right\} \quad \ldots\ldots(10)$$

in which ϵ has the value given by (6).

It follows herefrom that over a range, limited of course, but ample for our purposes, the colour change with varying θ_i for any particular component will be proportional to this fractional change in density; and, moreover, such colour changes for more than one component will be vectorially additive. We may therefore

analyse the variations ξ, η, ζ, ϖ linearly in terms of certain arbitrary unit contributions $\bar{\xi}_i, \bar{\eta}_i, \bar{\zeta}_i, \bar{\varpi}_i$ from the different components, i, and obtain :

$$\left.\begin{aligned} \xi &= \sum_{i=1}^{k} \theta_i \bar{\xi}_i, \\ \eta &= \sum_{i=1}^{k} \theta_i \bar{\eta}_i, \\ \zeta &= \sum_{i=1}^{k} \theta_i \bar{\zeta}_i, \\ \varpi &= \sum_{i=1}^{k} \theta_i \bar{\varpi}_i, \end{aligned}\right\} \quad \ldots\ldots(11)$$

where, for each component in the whole subtractive mixture, $\bar{\xi}_i$, $\bar{\eta}_i$, etc. are given by modified forms of (9) and (10) as follows:

$$\left.\begin{aligned} \bar{\xi}_i &= \{\alpha(B+C) - A(\beta+\gamma)\}(y/w\mathscr{D})^2, \\ \bar{\eta}_i &= \{\beta(C+A) - B(\gamma+\alpha)\}(y/w\mathscr{D})^2, \\ \bar{\zeta}_i &= \{\gamma(A+B) - C(\alpha+\beta)\}(y/w\mathscr{D})^2, \\ \bar{\varpi}_i &= \beta/\mathscr{D}. \end{aligned}\right\} \quad \ldots\ldots(12)$$

A, B, C, $\mathscr{D}$ here have the same significance as in (10) and α, β, γ are given by the following:

$$\alpha = \int_V u_i f E\bar{x}\, d\lambda,$$

$$\beta = \int_V u_i f E\bar{y}\, d\lambda,$$

$$\gamma = \int_V u_i f E\bar{z}\, d\lambda,$$

in which $u_i(\lambda) = -2{\cdot}3026 p_i e_i = -2{\cdot}3026 d_i$.

We are now in a position to ascertain the variation in colour which will result from any contemplated small variation, θ_i, as a limit imposed by practical conditions, for any component, i, in the mixture. The calculation, for each component, of the value of θ_i corresponding to the colour-discrimination limen, the liminal tolerance or any other specified limit requiring consideration, may also readily be made, the procedure involved being obvious without further elaboration.

The value of θ_i would be that corresponding to the radius vector of the liminal ellipsoid for the " direction of the colour variation " resulting from the form of variation $u_i(\lambda)$ in question.

When considering the assessment of tolerances in subtractive mixtures, causes of individual variations indicated by an analysis of the process may first

be separately investigated and the total limiting variations in p_i finally assessed on the basis of such an analysis. The corresponding total variation in colour may be found therefrom in accordance with the foregoing. Alternatively one may, either by virtue of available data applying in analogous circumstances or as the outcome of experience, be able to place a limit upon the total variation, θ_t, covering for one component all possibly contributory factors. From this the corresponding total colour-variation may be directly ascertained. Conversely, each individual variation, $\theta_i p_i$, corresponding to a stated colour tolerance may be calculated and subsequent reduction made in each such variation either on a probability basis or in some other systematic way, in order that the maximum total variation may lie within the prescribed limits.

In applying the above method numerically, summations are, of course, used and may be carried through rapidly with the aid of the condensed colorimetric tables (Smith, 1934), which will be found amply accurate.

§4. SUBJECTIVE APPRECIATION OF COLOUR TOLERANCES

Although, as indicated above, the specification of colour in terms of hue and saturation has little in common with colour measurement and does not therefore assist directly in the exact metrical specification of colour, the fact that variations of hue and saturation are appreciated and readily expressed in words by the large majority of people, whereas few could analyse the same colour variations at sight in terms of trichromatic units, causes this method to have an important use as a link between the specialist and those generally interested in colour. There is therefore an evident need for interpreting colour variations from one such system to another. Two different scales of purity or saturation are often used in this connection, viz. (*a*) the photometric scale defined in terms of luminosity, and (*b*) the colorimetric scale which postulates a quantity which is purely arbitrary, known as the excitation purity. Reference may be made to Hardy (1936) for further details.

It is, of course, largely immaterial what purity scale is adopted, as this is used merely to permit the conventional interpretation of variations which are subjectively appreciated; but it would seem that there can really be only two such scales of particular interest to colorimetrists and the world in general, viz., the photometric scale above referred to and a uniform-chromaticity scale. This latter is readily provided, to the accuracy assumed here for the expression of colour tolerances, by the R.U.C.S., and is that in which subjective analysis of such colour variations would be most directly appreciated and expressed. In this system the purity, p_χ, would have the value

$$p_\chi = \frac{u_s - u_w}{u_\lambda - u_w} = \frac{v_s - v_w}{v_\lambda - v_w},$$

where u_s, v_s, are the R.U.C.S. coefficients for the colour to be specified, u_w, v_w are the R.U.C.S. coefficients for the diluent radiation and u_λ, v_λ are the R.U.C.S.

coefficients for the point on the spectrum locus at its intersection with a straight line joining the points (u_w, v_w) and (u_s, v_s) on the R.U.C.S. colour chart and extended through the spectrum locus.

The means of effecting interpretation between any trichromatic system and hue and saturation may consist either of (1) graphs which represent, directly superimposed, the hue and saturation scales and some chromaticity scale or (2) tables made out for certain standard diluent radiations. The former method has been worked out in detail for white *C* as diluent by Hardy (1936): the latter method is adopted in a forthcoming publication (Hilger, 1941). The interpretation of colour tolerances in terms of variations of hue and saturation and the reverse interpretation may readily be made by such means and require no further consideration in detail.

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THE TECHNICAL ASPECT OF COLOUR TOLERANCE *

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ABSTRACT. The extent to which the characteristics of dyed materials are governed by the technical limitations of dyestuffs is discussed, also the various factors which have to be considered in the selection of dyes to produce a given result. The terms "shade" and "strength" as employed by the dyestuff-using industries are defined, and the methods are described by means of which these two factors are measured and controlled in the commercial production of dyed materials.

In his introductory remarks at the first meeting of the Physical Society Colour Group, the Chairman, Dr. Wright, indicated that the technical aspect of colour tolerance was concerned with the limits to which manufacturers can be expected to work, combined with a reasonable amount of reconciliation between public demand on the one hand and technical limitations on the other. As it would be impossible for me to deal with all the different types of colouring material in the time available, I propose to confine myself to one class—dyestuffs—since this group enters into the composition of an enormously wide range of manufactured products.

We find that the dyer is confronted with two kinds of limits to which he has to work. These may be classed as chemical and physical. The physical limits are concerned with the shade or hue of the dyed material, whereas the chemical limits are concerned with the other properties of the dyestuff. These are functions of the constitution of the dye, and determine its behaviour when subjected to the action of light, washing, acids, alkalies and various other factors. The extent to which each individual dye will withstand these various influences is carefully listed, and the complete list comprises what is known as the fastness properties of that due.

Now, most people think that a fast dye is one which will not fade on exposure to light. But fastness to light, while one of the most important, is by no means the only fastness which has to be considered by the dyer. There are a large number of influences to which dyes may be made fast, although it is very seldom indeed that any one dye is fast to them all. As a rule, fastness means expense,

* A paper read at the first meeting of the Physical Society Colour Group.

and one does not therefore specify more fastness properties than are necessary for the particular needs of the material to be dyed. Thus, fabrics intended for underwear should be dyed with a colour which is fast to washing, but there would be little point in making them fast to light, whereas a material for use in upholstery must possess good light-fastness, but need not be fast to washing.

Some kinds of material must possess the maximum possible fastness in certain categories. For example, cotton yarn used in the weaving of shirtings must, as well as being fast to light, be completely fast to boiling, since otherwise the colour would " bleed " when the shirt is laundered and would stain the adjacent whites. Worsteds for suitings must be fast to light and water, reasonably fast to washing, and also must be able to resist the action of organic acids—to withstand perspiration—and of alkalies, since splashes of road mud are often alkaline.

Another factor which influences the selection of a dyestuff is not concerned with its actual fastness properties, but with its method of application. This factor is taken into account in connection with the texture of the material. Thus, a tough, compact material, which is difficult to penetrate, must be dyed with a colour which can be prevented from fixing itself too rapidly, since otherwise the dye will be all on the outside of the material, and the inside of the threads will be left almost white. Pale shades in any class of material need this property also in the dyes used for them, as it would otherwise be difficult to obtain an even, level shade.

Most furs which require to be dyed are dressed with a material upon which hot water has a detrimental effect. In this case, therefore, a suitable dye is one which has an affinity for the hair when applied in cold solution.

There are, of course, further problems involved in the selection of a dyestuff for a particular purpose in order that the final result may be within the limits specified, but I think that enough has been said to give an outline of the nature of the chemical side of such limits.

Turning to the physical side, we have, it would seem, only one thing to consider, namely, the dyed material must match the pattern specified. It might be thought that this is a comparatively simple matter—merely a question of skill and practice on the dyer's part. Actually, there are one or two things which are not generally thought of in connection with matching.

You all know that a good many colours appear different when seen in artificial light. A careful selection of dyes has to be made, therefore, for materials which will be seen in both daylight and artificial light, since some colours exhibit a marked change, whereas others change to a very much smaller extent. Thus, a pattern could be matched with two different combinations of dyes. In daylight, all three would match perfectly. In artificial light, however, one would still match the pattern, while the other would appear completely different.

Another way in which the shade of a dye can vary is in what is called overhand and downhand viewing. Overhand viewing means looking at the material very obliquely, so that the light is reflected at a very obtuse angle, and downhand

viewing is the more normal method, where the reflection is much more acute. An instance where the overhand shade has to be considered is in the case of dyes for hats, since these garments are among the few which are commonly seen by both views. If the colour with which they are dyed shows a marked difference downhand to overhand, the hat will appear to be of two different shades.

Apart from such points as these, however, there is, of course, the main consideration—the actual physical match. Strange as it may seem in these days of colorimeters and accurate measurements, matching in a dyehouse is almost invariably carried out by the unaided eye. In arriving at a proper match, two factors are considered. These are known as shade and strength, and can, of course, vary independently. A definition of these two terms as used in the dyestuff world is here advisable, since there still exists, between the various colour-using industries, a certain amount of confusion as to their meaning.

By "shade", the dyer means what is often described as the hue of a colour. It is a function of the spectral composition of the light reflected from the dyed material, and describes the colour of this material in every respect except as regards strength.

The "strength" of a given shade is popularly known as "depth of colour", and is akin to the physicist's "degree of saturation." It is governed by the amount of dyestuff present in a given weight of material, and is expressed as a percentage dyeing of the given dyestuff. Thus, a "1 per cent dyeing" means, not that the material has been treated with a 1 per cent solution of the dyestuff, but that, for each 100 grams of material, 1 gram of dyestuff was present in the dye-bath. The reason for this is that dyestuffs normally possess an affinity for the material which they have to dye, and, therefore, under proper conditions, practically the whole of the colour present in the dye-bath is absorbed by the material. Hence it follows that, in the case of a piece of material dyed to a strength of 1 per cent, every 100 grams contain approximately 1 gram of dye.

So far as the dyer is concerned, the strength factor is purely a relative one, since it is dependent upon the concentration of the dyestuff as supplied by the manufacturers, and is, in practice, never reduced to a definite figure. It could, of course, be enumerated by a colorimeter in the same way as can a shade, but, as already mentioned, accurate measurements have so far been found unnecessary in the dye-house, since the eye is perfectly capable of judging a colour within the limits set by commercial tolerance.

An interesting fact concerning the strength of a colour affords one reason why measurement of this factor is unnecessary. It is well known that the ear is surprisingly uncritical when it comes to judging the relative strength of two otherwise similar sounds. The eye exhibits the same peculiarity in regard to the relative strength of two dyeings of similar shade. If those of you who are unacquainted with the dyeing industry were asked what was the smallest difference in strength alone which could be perceived by a close examination, you would probably think that a difference of one or two per cent could readily be seen.

In point of fact, however, few people here to-day could detect with any certainty a difference in strength of as much as 10 per cent. 15 per cent would probably be the minimum. Even a trained dyer, who can see differences completely undiscernible to the layman, cannot be sure of anything less than 5 per cent.

In the case of bright colours such as yellow, the discernible limit is even greater than this. It is far more difficult to see a 15 per cent strength difference in yellow than in green. When, therefore, a dyestuff manufacturer wishes to test the relative strength of two batches of yellow, he does not make a direct dyeing, but adds an equal amount of blue to each sample. Differences in the strength of the yellow will then be much more easily seen in the resultant green.

Turning now to the question of shade, there is little to be said about the tolerance obtaining under normal trading conditions. Competition is then such that this becomes of a very low order indeed as between two pieces of the same type of material, although a wider margin has to be allowed when matching two fabrics of widely differing textures, since, although isolated fibres from the two may match exactly, optical effects may make the two materials exhibit a seemingly considerable difference in shade.

A few facts upon the method of obtaining a match may, however, be of interest. The colour of a dyestuff is, of course, a subtractive one. The dyer's primaries are therefore red, yellow and blue, instead of the red, green and blue with which most of you work, since these are the only colours which cannot be produced by admixture.

The method of obtaining a given shade is therefore as follows:—The correct class of dyestuff is first selected, having regard, as already explained, to the nature of the material and the uses to which it will be put. If the colour required is composed mainly of a primary or secondary, the nearest dye to that is chosen, together with a suitable shading colour. In the case, however, of materials such as men's suitings, the shades are very often neutral in character, having no particular bias towards any colour of the spectrum. In this case, recourse is made to a mixture of the three primaries, and a large range of fawns and greys is available from them. It might be asked why a grey could not be obtained by using a suitably small amount of black dye, but the fact is that, apart from difficulties in application which would arise from the use of such a method, there is (with very few exceptions) no single dyestuff which is a true black: dyes used for black are always a very dark shade of a definite colour.

Reverting to those colours which consist mainly of a primary or secondary; the choice of a shading colour is determined in two ways. Suppose we have a green to match, and the nearest single colour is a little more yellow than the pattern, the obvious addition is a small amount of blue. If, however, the pattern is, say, olive in tone, a further variation is introduced, namely, relative brightness or dullness. If the pattern is brighter than any available dyestuff, nothing much can be done. It is very seldom that any addition can be made which will brighten the hue of a dye. To dull the shade is much simpler, and the rule to be

followed is well known. Any primary may be dulled by the addition of the secondary composed of the two remaining primaries; a secondary is dulled by the addition of the primary which is not one of its own components. Thus, according to the dyer's colour-scale, red is dulled with green, yellow with violet and blue with orange, and vice versa.

Briefly to sum up, the tolerances to which the dyer has to work are as follows: he must know the purposes to which his work will be put and select a suitable class of dyestuffs therefor, testing, if necessary, the degree of fastness of his pattern (where this is of the same type of material as that to be dyed) and choosing colours of equal ability in this respect. He must also make sure that the method of application of such dyes will suit the particular texture of the material. These factors will, of course, limit to a considerable extent the range of dyes available.

He must now consider the shade of the pattern, and decide which dye, if any, shall be used as the base shade. This being chosen, he will next decide on the nature and extent of the shading colour, or colours. One of these may be needed to change the shade of the base colour in the right direction, and the other to give the necessary dullness. Here, one or two trials with different proportions of the mixture will be needed. Finally, having arrived at the correct proportions, the amount of colour needed to give the correct strength must be found. In a good dye-house the result will be a perfect match.

It will be seen that the dyer has three independent variables to deal with. These are, I believe, often a bugbear to the mathematician, but a good dyer takes them in his stride, with nothing but his eye and a good light to aid him.

Whether this well-tried and old-established process could be expedited or improved upon by the adoption of modern scientific methods remains to be seen, and it may well be that the activities of this Group will throw some interesting light upon the problem.

NOISE IN RECEIVING AERIAL SYSTEMS

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ABSTRACT. Several authors have investigated theoretically and experimentally the signal/noise ratio in receiving aerial systems, but the problem of the validity of Nyquist's theorem for radiation resistance has hitherto not been satisfactorily resolved.

The problem is here discussed, and it is concluded that (i) for an aerial in an enclosure at uniform temperature, the radiation resistance is at that temperature from the point of view of noise, and thus (ii), for an aerial in free space, no noise originates in the radiation resistance. These results are shown to be consistent with the Rayleigh-Jeans radiation law, while the quantum-theory form of Nyquist's equation leads to the Planck radiation law.

The estimation of the noise occurring in practical aerial systems is discussed, consideration being given to the various external noise fields, viz. thermal radiation, Jansky noise, atmospherics. The actual noise received may be expressed conveniently by the equivalent temperature T_r of the radiation resistance. The general problem of evaluating the signal/noise ratio for any values of received circuit- and valve-noise is analysed and a criterion (K) of efficiency of an aerial system is deduced. The paper concludes with a numerical calculation of the performance of (1) a vertical aerial inductively coupled to a tuned-grid circuit and (2) a tuned-loop aerial, which are typical examples of an efficient and an inefficient system respectively.

§ 1. INTRODUCTION

In computing the signal/noise ratio in receiving systems, it is necessary to estimate the noise arising in the aerial circuit. Thermal noise is given by Nyquist's theorem, which states that the mean square fluctuation e.m.f. in the frequency band ν to $\nu+d\nu$ appearing in an impedance $Z(=R+jX)$ is given by

$$d\overline{e^2}=4kTR\,.\,d\nu, \qquad \ldots\ldots(1)$$

where k = Boltzmann's constant ($1{\cdot}37\times10^{-16}$ erg/deg.) (Nyquist, 1928). This formula directly associates the thermal agitation e.m.f. with the resistance, and the temperature T concerned is that of the resistive component of the impedance only.

Nyquist's proof of his theorem is a thermodynamic one, and is hence quite general and does not refer to the processes involved. However, Bernamont (1937) and Bakker and Heller (1939) have derived " mechanistic " proofs of the theorem by applying both classical and quantum statistics to the thermal agitation of free electrons in a conductor.

Moullin (1938) concludes on experimental grounds that " any dissipative element in a network is the source and initiation of thermal fluctuation voltage ". It seems certain, however, that this statement was not intended to apply to radiation resistance, for the latter, not being directly associated with any molecular dissipative process, requires separate consideration. Since the radiation resistance of an aerial is unlike other forms of resistance, e.g. it is independent of the temperature of the material of the aerial, and it does not involve any Brownian motion (as does ohmic resistance), it might be anticipated that radiation resistance is not of itself a source of fluctuation noise.

In this paper, however, it is shown that radiation resistance can apparently be the source of thermal noise, and it obeys Nyquist's theorem when the aerial is in radiative equilibrium with its surroundings.

§2. REVIEW OF PREVIOUS WORK

Previous literature contains only a few direct references to the validity of Nyquist's theorem for radiation resistance, though it is often implicitly assumed in the calculation of signal/noise ratio. Llewellyn in 1931 appears to have been the first to recognize the problem, and made the assumption that radiation resistance behaved like an ohmic resistance at atmospheric temperature. F. C. Williams (1937) has analysed the signal/noise ratio for the case of an aerial inductively coupled to a tuned-grid circuit, taking into account valve noise, while K. Fränz (1939) has made a detailed analysis for the case of a generalized coupling. Both writers, however, implicitly adopt Llewellyn's hypothesis, without giving due consideration to its validity.

Seki (1937) made measurements of noise in aerial circuits, tending to confirm the hypothesis, but they are by no means conclusive. Jansky (1937) points out that coupling an antenna to a tuned circuit should lower the latter's thermal noise since the dynamic impedance is reduced, whereas experiment always shows an increase of noise even when atmospheric and man-made interference are absent. Bell (1939) has discussed the problem theoretically and concludes that radiation resistance is not a source of thermal-agitation electromotive force, and that it should be treated as a reactance of equal magnitude when calculating the signal/noise ratio in aerial systems. The present writer does not agree with Bell's arguments, which are discussed below.

§3. AERIAL IN A UNIFORM TEMPERATURE ENCLOSURE

An aerial system will continuously radiate radio-frequency energy by virtue of the fluctuation currents flowing in it arising from its ohmic resistance. It will also receive energy by radiation at the same frequencies from the fluctuation currents in surrounding conductors. Thus fluctuation electromotive forces are induced in the aerial, and these may be attributed to thermal agitation in the radiation resistance, for, as will be shown later, the mean square electromotive force is proportional to the radiation resistance.

If the system is in thermal equilibrium in a uniform temperature enclosure at $T°$ K., then from the Prévost Theory of Exchanges a balance will exist between the energy radiated and that received, and furthermore, this equilibrium will exist at every frequency in the radiation spectrum. At the two terminals of the aerial let the impedance be $Z(=R+jX)$, which includes the ohmic resistance R and the radiation resistance R_r at the frequency ν. The radiation resistance R_r is defined as the ratio of the power radiated by the aerial to the mean square current at the terminals, producing the radiation.

By calculating the rate at which the fluctuations in the aerial radiate energy, and equating this to the power received from the external radiation, the electromotive force induced by the radiation can be found. Let this electromotive force have a mean square value of $d\overline{e_r^2}$ in the frequency band ν to $\nu+d\nu$. By reference to figure 1 it is seen that the fluctuation current flowing in the aerial due to its ohmic resistance R is given by

$$d\overline{i^2}=\frac{4kTR\,.\,d\nu}{Z^2},$$

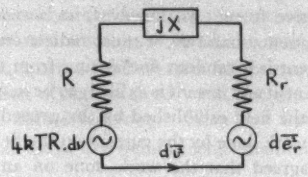

Figure 1. Equivalent circuit of aerial for noise.

and thus R supplies energy which is radiated at the rate

$$d\overline{P_{\text{rad.}}}=R_r\,d\overline{i^2}=\frac{4kTRR_r\,d\nu}{Z^2}.$$

For radiative equilibrium at each frequency, energy must be received at the same rate and supplied to the ohmic resistance R. This received power is

$$d\overline{P_{\text{rec.}}}=\frac{d\overline{e_r^2}}{Z^2}R.$$

For equality of the radiated and received powers it is seen that

$$d\overline{e_r^2}=4kTR_r\,.\,d\nu. \qquad \ldots\ldots(2)$$

The equivalent noise temperature T_r of the radiation resistance is now defined as that temperature which must be ascribed to R_r so as to give the value of the received-noise electromotive force when substituted in Nyquist's equation, i.e. in general such that

$$d\overline{e_r^2}=4kT_rR_r\,.\,d\nu. \qquad \ldots\ldots(3)$$

Thus an aerial in an enclosure at a uniform temperature, T, is in effect the source of a " Nyquist " thermal e.m.f. whose value corresponds to the condition $T_r=T$.

If the aerial temperature is T_a, not equal to T of the enclosure, radiative equilibrium does not exist, and there will be an unbalanced energy flow between the aerial and its surroundings in the sense which tends to equalize T_a and T.

It is concluded that the radiation resistance of an aerial in free space has an equivalent noise temperature of zero; expressed otherwise, there is no other source of radiation present to return energy to the aerial and thus to induce a fluctuation electromotive force. In calculating noise for an aerial in free space we can still use the circuit of figure 1, by putting $d\overline{e_r^2}$ zero. Bell's suggestion of replacing the radiation resistance by a reactance of equal magnitude is incorrect. Radiation resistance always behaves like a resistance, for it is " the ratio of the in-phase electromotive force to the current ", as Bell says. Replacing R_r by jR_r completely alters the impedance of the circuit and thus gives an incorrect value for the noise ; Bell used Williams's formula (1937) for the noise electromotive force, and it was merely necessary to put the temperature of the radiation resistance equal to zero in this formula, and leave the resistance unaltered.

Bell states that an aerial will not radiate energy when it carries fluctuation currents, and uses this argument to deduce that radiation resistance is not a source of thermal electromotive force. If, however, an aerial carries a mean square current $d\overline{i^2}$ in the frequency band $d\nu$, it must radiate energy at the rate $R_r . d\overline{i^2}$ even though the current is a random one arising from thermal agitation. Bell argues that " the current at any instant is as likely to be in opposition or quadrature as in like phase with the field established by the preceding current element, so that on the whole the work done by the current on the field is zero and no energy is radiated ". It is agreed that the work done on any current element (i.e. electron in motion) by the other electrons in the aerial will on the average be zero, for their field is randomly related to the motion of the electron considered. Work will be done, however, on each electron by its own field: on the classical theory, the power radiated by an electron with acceleration f is

$$P = \frac{2}{3}\frac{e^2 f^2}{c^3},$$

where e = electronic charge,
c = velocity of light *in vacuo*.

The essential difference between radiation from an aerial excited by a generator and one excited by fluctuation currents is that in the first case the electrons move in unison so that mutual radiation effects occur, while in the second only self-radiation by the electrons occurs. Thus in an aerial containing n free electrons, the power radiated in the two cases will be proportional to n^2 and n respectively. An exact analogue is in the scattering of x rays: for the heavier atoms and the longer wave-lengths, coherent scattering occurs and the intensity is proportional to n^2, while for light atoms and medium wave-lengths the intensity of the scattered radiation is proportional to n.

The relation $\overline{dP_{\text{rad.}}} = R_r \cdot \overline{di^2}$ thus holds for radiation by fluctuation currents, since the square of the current is also proportional to n and not to n^2, again because the electronic motions are randomly related.

§4. THE FREQUENCY DISTRIBUTION OF THE NOISE FIELD

It was seen that in a uniform temperature enclosure, an aerial has an induced noise electromotive force $\overline{de_r^2}$ of definite value, and it is of interest to calculate the intensity of the radiation field which excites it. The noise field in the enclosure will be perfectly diffuse, i.e. random in direction of propagation and polarization, and a relation between the effective height for diffuse radiation and the radiation resistance of the aerial may be readily deduced by the use of Poynting's theorem. The effective height h_e of the aerial can be defined as the ratio of the potential difference across the terminals to the electric intensity of the radiation field at the aerial, and it will be a function of the polarization and direction of arrival of the wave. The electric and magnetic intensity produced at a distant point by a current in the aerial may be expressed in terms of the current and the effective height of the aerial for the appropriate direction of propagation. The outward energy flow at that point is obtained from Poynting's theorem, and by integrating this over a closed surface surrounding the aerial, the radiation resistance may be derived. Let $\overline{h_e^2}$ be the square of the effective height meaned over all directions of arrival and polarization of the received wave, i.e. the mean square effective height of the aerial for diffuse radiation. Then at a frequency ν, the relation between R_r and $\overline{h_e^2}$ for an aerial in a homogeneous medium of dielectric constant κ and permeability μ is

$$\left.\begin{aligned} R_r &= \frac{8\pi^2\nu^2}{\kappa v^3}\,\overline{h_e^2}\ \text{(e.s.u.)} \\ &= 240\pi^3\sqrt{\frac{\mu}{\kappa}}\cdot\frac{\overline{h_e^2}}{\lambda^2}\ \text{ohms,} \end{aligned}\right\} \qquad \ldots\ldots(4)$$

where $\lambda = v/\nu$ = wave-length,

v = velocity of electromagnetic waves in the medium $= \dfrac{c}{\sqrt{\kappa\mu}}$.

The mean square electromotive force $\overline{de_r^2}$ induced in the aerial by the noise field will be related to the mean square electric intensity $\overline{dE^2}$ of the radiation by

$$\overline{de_r^2} = \overline{dE^2}\,\overline{h_e^2}.$$

Hence from equations (2) and (4)

$$\overline{dE^2} = \frac{32\pi^2 kT\nu^2}{\kappa v^3}\cdot d\nu.$$

The mean energy density of the radiation in the frequency interval $d\nu$ is thus

$$\left.\begin{aligned} \overline{d\epsilon_\nu} &= \frac{\kappa\,\overline{dE^2} + \mu\,\overline{dH^2}}{8\pi} = \frac{\kappa\,\overline{dE^2}}{4\pi} \\ &= \frac{8\pi kT\nu^2}{v^3}\cdot d\nu, \end{aligned}\right\} \qquad \ldots\ldots(5)$$

which is the Rayleigh-Jeans distribution law for black-body radiation. Thus the radiation produced by fluctuation currents in an aerial, and the radiation which induces the " Nyquist " noise e.m.f. in an aerial in a uniform temperature enclosure are to be identified with the usual temperature radiation.

The above analysis could have been presented in the reverse order by starting with the Rayleigh-Jeans radiation law, deducing the electric intensity of the radiation, and thus calculating the electromotive force which would be induced in an aerial in a uniform temperature enclosure. This would have been found equal to the value given by Nyquist's theorem for the radiation resistance at the temperature of the enclosure. Furthermore, the existence of a fluctuation electromotive force in ohmic resistances, of the value given by Nyquist's theorem, would be found necessary to give the balance of received and radiated power which occurs for an aerial in thermal equilibrium. This derivation of Nyquist's theorem for ohmic resistance would seem to be free from the objections which Moullin (1938, p. 39) raises in connection with Nyquist's original proof. It is not surprising that Nyquist's theorem and the radiation law should be consistent, for they may both be deduced by counting the number of degrees of freedom of electromagnetic vibration in a given frequency interval. In one case the system considered is one-dimensional, namely, a transmission line, while in the other it is a three-dimensional space.

The quantum theory form of Nyquist's theorem is

$$d\bar{e}^2 = \frac{4h\nu R \,.\, d\nu}{e^{h\nu/kT}-1} \qquad \ldots\ldots(6)$$

where h = Planck's constant ($6{\cdot}54 \times 10^{-27}$ erg. sec.). Equation (5) then takes the form of the Planck radiation law,

$$d\overline{\epsilon_\nu} = \frac{8\pi h\nu^3}{v^3(e^{h\nu/kT}-1)} \,.\, d\nu. \qquad \ldots\ldots(7)$$

However, the classical and quantum radiation theories are indistinguishable even at high radio-frequencies, for if

$$\nu = 10^8 \quad \text{and} \quad T = 300^\circ \text{ K.},$$

then

$$h\nu/kT = 1{\cdot}6 \times 10^{-5}.$$

This small value of $h\nu/kT$ means that the radio-frequency components of the temperature radiation form only a very small fraction of the total radiation. The total energy density of black-body radiation obtained by integrating (7) from $\nu = 0$ to ∞ is given by the Stefan-Boltzmann fourth-power law:

$$\epsilon = \frac{8\pi^5 k^4}{15c^3h^3} T^4 = 7{\cdot}65 \times 10^{-15} T^4 \text{ erg/cm}^3$$

For $T = 300^\circ$ K. this has the value

$$\epsilon = 6{\cdot}2 \times 10^{-5} \text{ erg/cm}^3$$

If the radio-frequency spectrum is taken as being from 0 to 1000 Mc./s. ($\nu = 0$ to 10^9), its contribution to the energy density is

$$\Delta\epsilon = \frac{8\pi kT}{c^3}\int_0^{10^9} \nu^2 \,.\, d\nu = 1{\cdot}28 \times 10^{-17} \text{ erg/cm.}^3,$$

or only about $\dfrac{1}{5 \times 10^{12}}$ of the total!

§5. THE PRACTICAL PROBLEM

The communication engineer is interested in the problem of the maximum signal/noise ratio obtainable with a given receiving aerial system. The one component of noise e.m.f. which can be calculated with certainty is that due to thermal agitation in the ohmic resistance R of the aerial system, and is given by Nyquist's theorem:

$$\overline{de^2} = 4kTR \,.\, d\nu,$$

where T is the temperature of the aerial.

Unfortunately the discussion above of an aerial in a uniform-temperature enclosure is of little assistance in the computation of the noise e.m.f. in an aerial system erected on the surface of the earth. In the first place, the temperature radiation at the earth's surface is not black-body radiation, nor is the aerial temperature any guide to the actual noise field existing, and secondly, there are other natural (i.e. not man-made) noise fields which can be far in excess of the thermal noise field. If an aerial has an equilibrium temperature T governed purely by radiation of spectral distribution ϵ_ν and $a(\nu, T)$ and $e(\nu, T)$ are its absorptive and emissive powers, the equilibrium equation is

$$\int_0^\infty [a(\nu, T)\epsilon_\nu - e(\nu, T)] \,.\, d\nu = 0. \qquad \ldots\ldots(8)$$

Only if ϵ_ν follows a black-body distribution will the integrand be zero at every frequency; in other cases there will be no definite relation between the radiation field at any particular frequency and the equilibrium temperature. This is especially the case for radio frequencies, for as we saw above, the energy density at these frequencies is an extremely small fraction of the total, and hence they play an insignificant part compared with the heat or light frequencies ($\nu = 10^{14}$ to 10^{16}) in determining the equilibrium temperature.

In practice, conduction and convection exercise a large influence on the equilibrium temperature of the aerial by tending to equalize it to that of its immediate surroundings. Presumably, the most important source of thermal radiation at the earth's surface is the earth itself, especially in the radio-frequency spectrum, remote from the peak in the energy-distribution curve. Despite the relatively high temperature of the sun (6000° K.), its distance is so great that it cannot contribute appreciably to the noise field on the earth. This is confirmed by Jansky (1935), who states that no solar radiations at radio frequencies can be detected.

The intensity of illumination by starlight is very small compared with that from the zenithal sun, so that stellar thermal noise will be quite undetectable. In a series of elaborate experiments, however, Jansky (1932 to 1937) has established the existence of a noise field whose source is situated in the Milky Way; since we have concluded that thermal noise cannot be responsible, another mechanism must be sought, and G. Reber (1940) has recently suggested an electronic process which accounts for the noise. This interstellar radiation depends upon the directional characteristics and orientation of the aerial, on the time of day and of year, and on ionospheric conditions. Jansky's papers should be consulted for further information, but it may be stated that the noise energy observed on $\lambda = 16{\cdot}7$ m. using a rhombic aerial was always more than 8 db. above (6 times) first-circuit noise, and on occasions reached 31 db. above (1300 times) this level.*

Whatever be the actual value of the noise e.m.f. ($\overline{de_r^2}$ for a band-width B) induced in the aerial from external sources, it may be taken into account by attributing the appropriate temperature T_r to the radiation resistance R_r:

$$\overline{de_r^2} = 4kT_rR_rB. \qquad \ldots\ldots(9)$$

T_r will generally be independent of the band-width B (Carson, 1925), but it will be a function of the aerial directivity if the noise radiation is not perfectly diffuse (e.g. interstellar or atmospheric noise). Jansky's measurements have shown that on short waves, even in the absence of atmospherics, T_r is always greater than T, and can exceed 1000 T in unfavourable conditions.

In most receiving systems the aerial is coupled by an intermediate network to a tuned circuit at the grid of the first valve in the receiver, and it is the signal/noise ratio at this point which is required. The evaluation of this ratio may be treated quite generally; let the aerial be connected to the terminals (1, 1) of a linear, passive network at whose terminals (2, 2) it is desired to know the signal/noise ratio (figure 2).

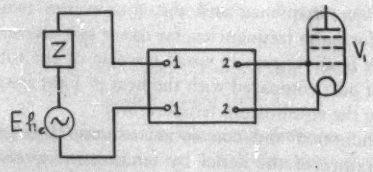

Figure 2. Generalized aerial-grid circuit coupling.

The mean square noise e.m.f. at (2, 2) is given by

$$\overline{e_n^2} = 4kB[n_A^2T_rR_r + T\Sigma n_s^2R_s], \qquad \ldots\ldots(10)$$

* Another very variable source of noise radiation is "atmospherics", i.e. radiations produced by electrical disturbances of the atmosphere. These are not amenable to any except a very rough estimation based on previously collected data (Potter, 1931 and 1932).

where R_s = resistive component of the sth impedance element Z_s of the network, excluding the radiation resistance of the aerial;
n_s = modulus of voltage transfer ratio from Z_s to (2, 2);
n_A = modulus of voltage transfer ratio from aerial to (2, 2); and
T = temperature of the ohmic resistances in the network.

Valve noise may be taken into account by adding the term $4kBTR_v$, where R_v is the equivalent noise resistance of the first valve:

$$\overline{e_n^2} = 4kB[n_A^2 T_r R_r + T\Sigma n_s^2 R_s + TR_v]. \qquad \ldots\ldots(11)$$

If the aerial receives a signal of field intensity E, for whose direction of arrival and polarization the effective height is h_e, the signal potential-difference at (2, 2) is

$$e_s = Eh_e n_A.$$

Thus the signal/noise ratio after amplification by the first valve is

$$\rho = \frac{e_s}{\sqrt{e_n^2}} = \frac{Eh_e n_A}{\sqrt{4kB[n_A^2 T_r R_r + T\Sigma n_s^2 R_s + TR_v]}} = \frac{\rho_0}{\sqrt{1+\frac{1}{K}\frac{T}{T_r}}}, \qquad \ldots\ldots(12)$$

where

$$\rho_0 = \frac{Eh_e}{\sqrt{4kBT_r R_r}} = \frac{\text{received signal}}{\text{received noise}}$$

is the absolute maximum of the signal/noise ratio, and

$$K = \frac{n_A^2 R_r}{R_v + \Sigma n_s^2 R_s}$$

is a measure of the overall efficiency of the receiving system comprising the aerial circuit and the first valve. The greater the value of K, the more nearly does ρ approach its limiting value of ρ_0. As an arbitrary criterion, it might be stated that if $K \geqslant 0{\cdot}1$, the system can be considered efficient.

To interpret these results in a practical form, a field strength E_1 can be specified such that the signal/noise ratio is unity after amplification by the first valve. From equation (12)

$$E_1 = E_0\sqrt{\left(\frac{T_r}{T}+\frac{1}{K}\right)}, \qquad \ldots\ldots(13)$$

where

$$E_0 = \sqrt{(4kTB \,.\, R_r/h_e^2)}. \qquad \ldots\ldots(14)$$

From equation (13) it is seen that the signal/noise ratio reaches its maximum (i.e. E_1 reaches its minimum) when K is a maximum, and the optimum coupling condition for this is clearly independent of T_r/T. Two cases will be considered:

(1) A vertical aerial coupled by a mutual inductance M to the tuned-grid circuit L, R, C (figure 3). If the aerial is not too short, the primary impedance when made non-reactive is practically wholly R_r, and it is then found that

$$K = \frac{r}{1+R_d(1+r)^2/D}, \qquad \ldots\ldots(15)$$

where $r = \omega^2 M^2/R_r R$ and D = dynamic impedance of the grid circuit with the aerial disconnected.

The optimum value of r is

$$r_{\text{opt.}} = \sqrt{(1+D/R_0)}, \qquad \ldots\ldots(16)$$

for which K reaches its maximum,

$$K_{\text{max.}} = \tfrac{1}{2}(\sqrt{(1+D/R_0)}-1). \qquad \ldots\ldots(17)$$

The optimum coupling is thus always closer than that for maximum signal, viz. $r=1$.

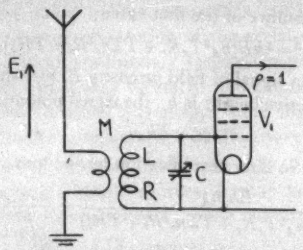

Figure 3. Vertical aerial receiving circuit.

To illustrate these results numerically, consider typical values:

$B=5$ kc./s., $\quad T=290°$ K.,

$R_0/D=0{\cdot}1$, $\quad \lambda=20$ m. (15 Mc./s.).

For a vertically polarized ground-wave E_0 will then have the value 0·0177 μv./m. The variation of E_1 with the coupling r is shown in figure 4 for the cases of

(i) $T_r/T=0$: Free-space condition, giving absolute minimum of E_1.
(ii) $T_r/T=1$: Uniform-temperature enclosure at T.
(iii) $T_r/T=10$: Typical case of low level of received noise.
(iv) $T_r/T=1000$: Typical case of high level of received noise.

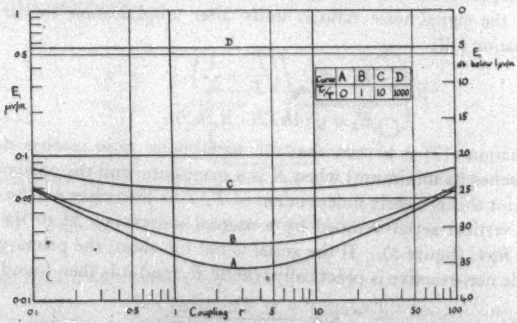

Figure 4. Receiving characteristics of vertical aerial circuit.

From the curves it is concluded that the signal/noise ratio is not critically dependent on the coupling, even for quite low levels of received noise ($T_r/T=10$).

The curves are representative of a typical case of a high value of K (here ranging from 0·1 to 1·2) which obtains in aerial systems whose radiation resistance is not small compared with the ohmic resistance. In these circumstances the received noise predominates over the circuit and valve noise.

(2) A tuned loop of dynamic impedance D, in which the radiation resistance R_r is small compared with the ohmic resistance R, connected directly in the grid circuit of the first valve. For this case

$$K=\frac{R_r}{R}\cdot\frac{1}{1+R_r/D}. \qquad \ldots\ldots(18)$$

For a loop 0·5 m. square of ohmic resistance 3 ohms, the other parameters being the same as for the vertical aerial case, it is found that $K=0{\cdot}0072$.

The table shows for comparison the values of E_1 for

(*a*) the ideal limiting case of an aerial having no ohmic resistance connected directly to a noiseless valve (i.e. $K=\infty$);
(*b*) the vertical aerial with optimum coupling; and
(*c*) the tuned loop, when its plane is in the position for maximum reception.

It is seen that the loop is a poor receiving system, since in all but high levels of received noise the thermal and valve noise predominate and thus set a relatively high lower limit for E_1. This is typical of aerial systems having a low value of K.

E_1 in μv./m.

$T_r/T=$	0	1	10	1000
Ideal system ($K=\infty$)	0	0·0177	0·056	0·56
Vertical aerial with optimum coupling ($K=1{\cdot}16$)	0·0163	0·0241	0·058	0·56
Tuned loop ($K=0{\cdot}0072$)	0·208	0·209	0·215	0·66

§ 6. ACKNOWLEDGEMENTS

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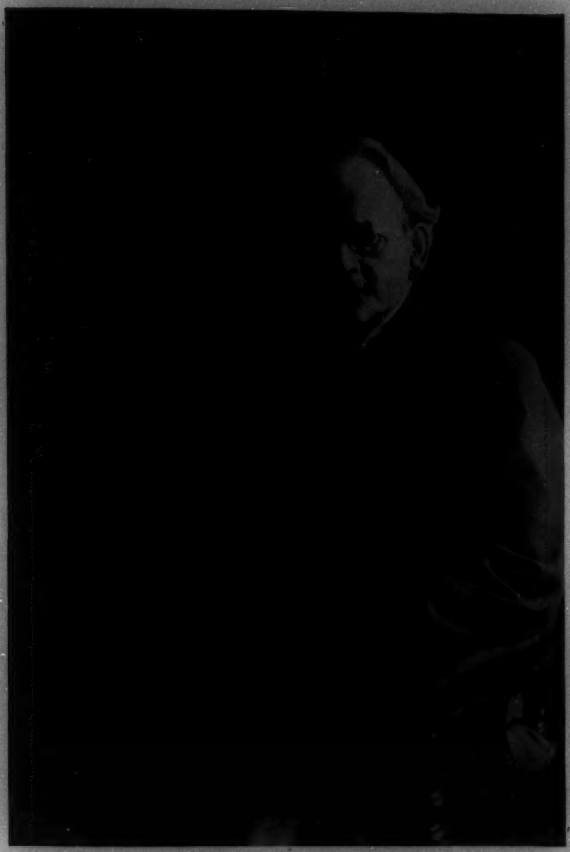
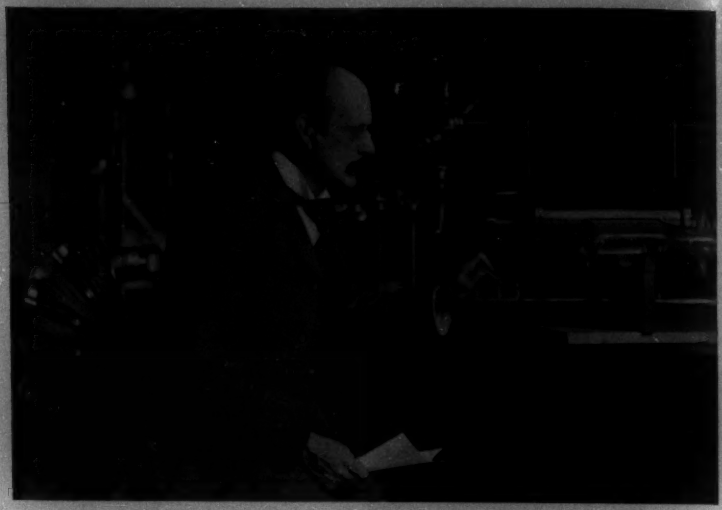


SIR JOSEPH THOMSON, 1928.
(Reproduced by courtesy of Lady Thomson.)



Experiments on e/m, about 1902.
(Photograph by W. H. Hayles: reproduced through the courtesy of Dr. G. F. C. Searle.)

Measurements of positive-ray parabolas, about 1912.
(Photograph reproduced through the courtesy of W. H. Hayles.)

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OBITUARY NOTICES

SIR J. J. THOMSON, O.M., F.R.S.

In the description of his life which Thomson has published under the title "Recollections and Reflections" he emphasizes the fact that the period between his birth, 18 December 1856, and the present day had been one of the most eventful in the history of the world: in politics, economics, science and technology amazing changes have happened. His own life, however, did not reflect this revolutionary character of the surrounding world, but went rather in normal steps, devoid of exciting events apart from those occurring in that sphere on which his interest was concentrated.

He mentions that he was destined by his father, a bookseller in the Manchester suburb of Chatham, to become an engineer. He was sent, at the age of fourteen, to Owens College, an institution founded in 1846 by a Manchester merchant, which developed later into the present University of Manchester. On the staff of this school were brilliant scientists—to mention only two names: Osborne Reynolds for engineering, H. E. Roscoe for chemistry. These men aroused Thomson's interest in science. But the deciding event which deflected him from becoming an engineer was his father's death. In these days the only way to engineering was by apprenticeship. Thomson's mother could not afford the money for this purpose, and so he remained at Owens College, where he had just gained scholarships. Here he was introduced to physics by Balfour Stewart, under whose influence he wrote his first paper (on a question concerning the transformation of different forms of energy into one another).

Thomson obtained an Entrance Scholarship for Cambridge, and entered that university in 1876, never to leave it again. Here he had the most distinguished teachers. It is remarkable that the man who was to become one of the greatest experimentalists had, as a student, read chiefly mathematics. He attended lectures by Routh, Cayley, Stokes and Adams. He also thoroughly enjoyed the gay life of the undergraduate. In 1880 he passed the Mathematical Tripos as second Wrangler, Larmor being his senior.

Thomson became fellow of Trinity in 1880, won the Adams Prize two years later, was soon appointed University Lecturer, and elected Fellow of the Royal Society in 1884 when only 28 years of age. The work by which he gained these distinctions was still purely mathematical, but it was during this time that he started experimental work at the Cavendish Laboratory.

Until the middle of the nineteenth century, research laboratories did not exist at the universities. The professors carried out their experiments in their private houses. It was in 1869 that the first plans were considered for a university

laboratory of physics in Cambridge, and it was officially opened in 1874 under James Clerk Maxwell. Maxwell, who died young, was succeeded by Lord Rayleigh, who resigned in 1884. And now the amazing thing happened; for the young mathematician Thomson, who had only just started doing experimental work in the Cavendish, was elected Cavendish Professor and Director of the laboratory.

It was an audacious decision but most successful, as soon became evident. For Thomson went with all his energy, and a quite unbiased mind, into his new job, and made his laboratory into one of the great centres of research which attracted pupils from all over the world. The list of his collaborators contains celebrated names from many nations, and his pupils today occupy many important chairs in Great Britain and the Empire. I was myself his pupil in 1907, and came under the spell of his personality. He was not a brilliant lecturer in the more technical sense. But his lectures were impressive by the crystalline clearness of his explanations, and the simplicity and beauty of his experiments.

There is not much more to record about Thomson's life. He was several times in the United States and in Canada; he played an important part during the last war as adviser to the Government on technical questions. In 1918 he retired from his chair and became Master of Trinity. His successor was Lord Rutherford. In the Lodge of Trinity he spent a peaceful but still active old age, never ceasing to do some research. His greatest satisfaction was certainly the day when his son received the Nobel Prize, one of the few cases where father and son have been independently honoured in this way.

If we now turn our attention to his work there are a few remarkable features, one of which I have already mentioned, namely, that Thomson started as a mathematician and developed, in a rapid transformation, to a skilled experimentalist. The opposite process is not so rare; there are several examples of men who started as experimenters in a narrow field which widened under their own influence, and who were, step by step, compelled to use deeper mathematical methods in order to digest their own results. Now, if Thomson became a leader in experimental physics, it does not follow that he was extraordinarily clever as a manipulator. In fact, he did very little with his own hands. He pondered over the experimental possibilities, found a straightforward method, designed an apparatus in his head, and left the actual performance to his skilled assistants and mechanics.

Thomson's work is typical of the conception of science in the nineteenth century. Observation and collection of facts, and the mathematical interpretation of these, were still based on the principles laid down by Newton. It was this kind of unsophisticated Natural Philosophy which formed the background of Thomson's research. That his results, together with those of great contemporaries—Hertz, Becquerel, Röntgen, the Curies, Rutherford—should become the foundation of a new type of Natural Philosophy, as characterized by the words Relativity and Quantum Theory, was not a motive of his endeavours.

One of Thomson's first papers, his thesis for the fellowship of Trinity, was

devoted to a generalization of a well-known " reciprocity theorem " of dynamics to more general cases which occur in physics and chemistry; an example is this:—If the magnetization of iron decreases with rising temperature, then the theorem states that iron will be heated if its magnetization is increased. Phenomena of this kind are numerous in physical chemistry.

The centre of interest in physics at this time was the theory of electricity and magnetism. Thomson was strongly influenced by Maxwell's writings, chiefly by his *Treatise on Electricity and Magnetism*; a later book of his own appeared under the title *Recent Researches*, as a kind of continuation of Maxwell's work. But, strangely enough, Thomson's principal work went in a direction where not the field but the charges were the object of the interest. It was the discovery and investigation of atoms of electricity, or electrons, which made Thomson's fame. He did not, of course, start on his scientific journey with the idea of this discovery in his mind. He simply followed the trend of his time to investigate all kinds of electric phenomena occurring in gases. The motive for this work was the conviction that the properties of gases are simpler than those of the denser modifications of matter, and well described by the kinetic theory.

Thomson devoted an enormous amount of work to observations and measurements on the passage of electricity through gases, and collected all his results in a celebrated book, still one of the standard works on the subject. He found that the action of X rays in making a gas conductive is due to ionization. The question of what cathode rays really are was also a subject of lively discussion for many years. Opinions were divided. The German physicists, with the significant exception of von Helmholtz, regarded them as waves of a similar kind to light; the English physicists without exception thought they were negatively charged particles.

In 1894 Thomson succeeded in measuring the velocity of the rays with the help of a rotating mirror, and found a value of about 1/2000 of that of light. This made it very difficult to consider the rays as other waves of the same kind as light. Then Perrin showed that a cylinder in front of the cathode was charged negatively when struck by the rays, and Thomson extended this result to the case where the rays were deflected by a magnetic field.

In this way it was conclusively demonstrated that the cathode rays were particles charged with negative electricity. The last obstacle was removed when Thomson disclosed the reason why Hertz's experiments to deflect the rays by electric fields had failed: the cause was the use of an inadequate vacuum, which produced a screening volume-charge of ions, so that the external field did not reach the ray.

The determination of the nature of these particles was achieved by a series of experiments in which Thomson used an electric and a magnetic field simultaneously. The specific charge of the particles was found to be independent

of the nature of the gas and of the material of the cathode, and its value 2000 times the corresponding quantity of the hydrogen atom as determined by electrolysis.

The problem of the absolute magnitude of the charge was also attacked in the Cavendish. The method used for this purpose is due to C. T. R. Wilson, who brought it to the highest degree of perfection in his celebrated cloud chamber. The result was the proof that there exists a smallest unit of electric charge, which is the same under all circumstances for ions of every kind and origin. From this one must infer that the charge of the cathode-ray particle and that of the hydrogen ion must be identical. Hence Thomson's result means that the cathode-ray particles must have a mass about 2000 times smaller than the mass of the hydrogen atom.

When J. J. Thomson announced this surprising result in a lecture given to the Royal Institution, he was clear about its far-reaching meaning. He devoted a great deal of work to showing that these particles, later called electrons, are identical whatever their source, that is, the material of the cathode and the kind of gas in the tube. He came to the conclusion that the atom is not indivisible, as its name implies, but that he had found a sub-atom, a primordial element which was a common constituent of all atoms.

Now the discovery of the electron put the problem of the primordial element for the first time on an empirical basis, and the way was open for " constructing atoms " out of electrons. There was, however, a difficulty: all electrons are negatively charged, the whole atom is neutral,—where is the positive electricity? We have today a definite answer to this question, based on experimental facts. But before these were known the way was open to speculation. Thomson himself constructed an atomic model which was extremely clever and represented some features of the behaviour of real atoms quite satisfactorily. He assumed that the positive electricity is continuously and uniformly spread over the interior of a sphere, and that in a neutral atom a number of electrons are swimming in the positive sphere under the action of the central force and their mutual repulsions. He showed that these electrons would arrange themselves in regular configurations, rings, tetrahedrons, etc., about which they would perform harmonic vibrations. The possible arrangements had a striking similarity to the periodic system of the elements, and the vibrations at least the right order of magnitude for the optical frequencies in line spectra.

The real solution of the problem of the nature of the positive charges in the atom came not from speculations but from experiment. Rutherford, later Thomson's successor in the Cavendish chair, discovered the methods for attacking this problem by radioactive bombardment and the observation of its effect. The Rutherford-Bohr model of the atom is the result of the most efficient collaboration of experiment and theory. On the experimental side Thomson has also contributed to this success. The problem was that of the number of electrons in the atom, and it was important for his own atomic model and that of Rutherford as well. There are different methods of counting the electrons,

all using the scattering of incident rays by the atoms. Thomson considered x rays and showed that the ratio of the scattered intensity to the incident one was proportional to the number of electrons in the atom, with a factor depending only on universal constants—the charge of the electron and the velocity of light. Thomson's scattering formula is still the basis of all modern generalizations. His result, based on the measurements of Barkla, was that the number of electrons was about equal to the ordinal number of the atom in the periodic table of elements. This was confirmed by Rutherford and Chadwick's determination of the nuclear charge, which must be identical with the total charge of the electrons. The imposing edifice of modern atomic theory rests upon this work of the two late heads of the Cavendish Laboratory.

But with this theory of the electronic structure of the atom the fundamental problem of the primordial particle is not yet solved. For each atom has its particular nucleus, and their number is uncomfortably large. No reduction had so far been achieved. Thomson also took a deciding part in the solution of this problem. This solution was prepared by some facts of radioactivity. It was suggested by Soddy that amongst the unstable, exploding atoms there are some which have nuclei of different mass, but equal charge, so-called isotopes. Now the problem arose whether there were not isotopes also amongst the common, non-radioactive atoms. It was again Thomson who developed methods for proving this. He showed how, by deflecting atomic rays in electric and magnetic fields, one can separate all kinds of atoms in the beam according to their weight and charge. In this way he discovered the existence of isotopes for the gas neon and later for chlorine. The method, somewhat modified, was brought to an incredible perfection by Aston.

In this way Thomson played a deciding part in the greatest simplifications for our conception of matter. This was chiefly due to the directness and simplicity of his experimental arrangements, but also, to some extent, to his mathematical gift.

Thomson was, in his scientific attitude, a remarkable mixture of revolutionary and conservative. He took nothing for granted, but he clung to classical ideas. He had his own kind of mathematics which he had learned in his youth, and he did not bother to assimilate new methods. He appreciated Einstein's theory of relativity and its far-reaching consequences, but he preferred to consider it not as a new principle, but as an intrinsic property of Maxwell's equations of the electromagnetic field. He continued to consider the ether as the seat of the electromagnetic phenomena, and to use it as if it had a reality of the same kind as any matter.

Concerning quantum theory, Thomson's standpoint was that of a sceptical onlooker. But this changed when his son discovered the experimental proof of the wave character of the electron. It is a fascinating fact that father and son have given the most striking evidence for the apparently contradictory properties of the electron: the father proving its character as a particle, the son its character

as a wave. Modern quantum theory has shown that these are two aspects of the same phenomenon, depending on different kinds of observation: not contradictory, but complementary. Thomson was extremely proud of his son's success, and tried to assimilate the new results to his old convictions.

As mentioned above, I was Thomson's pupil in 1907 and deeply impressed by his lectures. I did not see him during that period in which the war had produced a deep gulf between the European scientists. It may have been in 1924 or 1925 when I again came to Cambridge and met Thomson's son, who is roughly my contemporary. He took me to the Cavendish and urged me to see his father. So we entered a darkened room full of the paraphernalia of physical research—tubes, wires, pumps in a bewildering mass. There was the old man standing, watching some bright phenomenon in a tube. His son introduced me: "Here, father, is your former pupil, who wishes to meet you again, etc." J. J., as he was called, even by his son, turned round: "How do you do; look here, this is the spectrum . . .", and so he jumped immediately to the problems which occupied his mind, disregarding all personal and historical events which had separated us for so many years.

His autobiography contains many good examples of his art of telling a story. He liked particularly to compare the modern life of the student at Cambridge with his own time, not to the advantage of our days. Once he pointed out to my wife a feature which seemed to him especially depressing: "In my day", he said, "it was not good style to ask a tailor for the price of a suit if you ordered one. You simply paid—or you didn't. The prices were fixed in such a way that it did not matter if a certain percentage of people did not pay their debts. But today they not only ask the price, they even leave the shop and go to a cheaper one!" But he himself was extremely modest—the opposite of a dandy. When I lived in Cambridge he was deeply moved by the fate of the wife and daughters of the great Heinrich Hertz, who had to leave Germany. He did all he could for them and for many others.

Thomson had a great interest in personalities. He met innumerable men of all descriptions in his long life, and his *Recollections* contain vivid descriptions not only of contemporary scientists, but also of many other people. He gives an attractive account of some of his predecessors in the Master's Chair at Trinity, and of many prominent Trinity men, amongst them men of world-wide fame, as the Indian mathematician Ramanujan and the poet Housman, and others of more local importance. Thomson loved his college, and I think that the last years spent in Trinity Lodge were as happy as any other part of his long life. He died at the age of 83 and was buried on 4 September 1940 in Westminster Abbey.

MAX BORN.

SIR J. J. THOMSON, Master of Trinity College, Cambridge, and honorary professor of physics in the University, died on 30 August 1940. The months which intervene have made no easier the task of writing a worthy memoir of him. It is, perhaps, too early to attempt a formal estimate of the importance of his life and work. The fresh impetus which he gave to physics has by no means exhausted itself; nor have the men whom he trained completed their work—though most of us are now middle-aged, or at any rate like to think of ourselves as such. The attempt would, in any case, be impossible for one who, for twelve happy years, had the privilege of working under him, and who owes to him most of what he knows of physics and affairs. As one sits and ponders, memories come flooding in which will not be denied. Thomson could not have come short of greatness in any walk of life. It was the good fortune of physics to have attracted his allegiance.

Born near Manchester on 18 December 1856, Joseph John Thomson, after spending some little time at Owens College, Manchester, entered Trinity College, Cambridge, in 1876, an association destined to remain unbroken for 64 years. As was usual at the time for prospective Natural Philosophers, Thomson read for the mathematical Tripos, which he took in 1880. He was second wrangler (Larmor being senior wrangler that year), and he also gained the much coveted Smith's prize. But although, as we are sometimes rather unkindly reminded, Thomson was a mathematician before he was a physicist, and although, throughout his career he always liked doing his own mathematics, it was clear from the trend of his immediate research work that he regarded his mathematical training merely as a preparation for an attack on the mysteries of physics. His *Essay on Vortex Motion* was a brilliant piece of mathematical analysis, but behind it lay the thought that in motion of this kind might be found the clue to the mystery of matter. The researches collected in his book on the *Application of Dynamics to Physics and Chemistry* (published in 1886) showed clearly where his interests lay.

They also showed that in the young Thomson, Cambridge had a mathematical physicist of great originality and unusual promise. When the Cavendish professorship of experimental physics fell vacant in 1884, by the resignation of the late Lord Rayleigh, the electors to the chair probably had little difficulty in deciding that Thomson was the right man to fill the vacancy, though, if rumour is to be believed, they seem to have felt some not unnatural trepidation as to how their nomination would be received. Even such a liberal-minded university as Cambridge might be expected to show some mild reaction when a twenty-seven-year-old mathematician was elected to succeed such intellectual giants as Maxwell and Rayleigh in a chair of *experimental* physics. All the world knows now how the courage of the electors was justified.

As was not unnatural, Thomson's early days in the professorship were occupied with extending and consolidating the electromagnetic theories of Maxwell, work which was embodied in *Recent Researches in Electricity and Magnetism*,

published in 1893. By this time, however, Thomson had convinced himself that the problems still outstanding, and in particular the all-important problem of the relation between electricity and matter, were not to be solved by analysis alone—recourse must be had to other and more physical methods. In particular, he felt, following a suggestion which he ascribes to Maxwell, that the phenomena attending the discharge of electricity through gases at low pressure might well repay close study, and to this he now turned his attention.

It is sometimes said that Thomson was not a great experimenter, and it is true that he never practised those manipulative arts which all his students were obliged, not without sweat and tears, to acquire if they wished to make progress with their researches. But Thomson, like Faraday, loved an experiment ; and in the higher qualities which mark the great experimenter—the ability to realize experimental difficulties, and to devise methods (often as startling in their novelty as in their simplicity) for overcoming them—he was supreme. He had, too, a quite uncanny flair for putting his finger on the weak spot in an experimental lay-out, and a research student whose apparatus " wouldn't work " rarely appealed to him in vain. All who watched the delightful experiments and demonstrations with which he illustrated his annual courses of lectures at the Royal Institution (where he was appointed professor of natural philosophy in 1905) could easily realize that in his power of planning original and effective experiments, whether to drive home a point in a lecture, or to solve some knotty problem in research, he fell little short of Faraday himself. The manipulations were the manipulations of Everett (his personal assistant for so many years), but the ideas were the ideas of Thomson.

The discovery of the electron was announced by Thomson at a Friday evening discourse at the Royal Institution on 30 April 1897. Some slight question has been made from time to time as to Thomson's absolute priority in the discovery; and it is true that there were other workers in the field, and that the discovery could not long have been delayed. There is, however, a very real sense in which a discovery in science belongs to the man who knows what he has found, and what to do with it. On these grounds alone, even if there were no others, science would be right in regarding Thomson as the " father of the electron." He was the first to realize that what had occurred was not a mere addition to knowledge; it was a revolution, destined to shake physics to its very foundations. To change the metaphor, a gateway had been suddenly opened into new territories of vast, though unknown potentialities; and through this gateway Thomson marched to take possession, accompanied by the small band of research students who were beginning to gather round him in the Cavendish Laboratory.

The group, in the year which saw the discovery of the electron, was small in comparison with what it was destined to become in later years. At that time research was regarded as a personal idiosyncrasy rather than as the normal completion of a university training in physics. In fact Maxwell, some years earlier, had seriously doubted whether a university graduate could ever be expected

to submit himself to the insufferable tedium of experimental research work. But it included, among others, Ernest Rutherford, J. S. Townsend, and C. T. R. Wilson, and was quickly joined by Langevin, Zeleny, Barkla, H. A. Wilson, and O. W. Richardson, to mention but a few of these who shared in the first discoveries. Inspired and directed by Thomson, and basing their work on his pioneer discoveries of the electron and the gaseous ion, they set out to explore in many directions the new and fertile fields of research into which he had led them. Conduction through gases, radioactive radiations, photoelectricity, thermionics, and the properties of x rays were among the subjects which sprang into existence or were vastly extended and remodelled during this amazing period of activity. To attempt to summarize the work proceding under Thomson's direction at this time would be to write a history of physics in one of its most formative periods—a task far beyond the scope of this simple memoir. Much of it is recorded in the first edition of Thomson's own book, *Conduction of Electricity through Gases*.

This may be the most appropriate place in which to give some brief account of Thomson's second great service to science, and one which lay even nearer to his heart than his own scientific work—the foundation and upbuilding of the Cavendish school of research. I have no doubt that, with his quite unfeigned modesty about his own scientific exploits, he regarded this as his main life-work, and to it he devoted himself unsparingly. Thomson was more than a great scientist: he was a great teacher, and a great inspirer of men. The fame of his discoveries attracted to him a steady stream of eager young students from every part of the civilized world, and to them he gave himself unstintedly. The interests of his research students, both intellectual and material, were his intimate concern. He shared with us freely his inspiration, his ideas and his time. In his advanced lectures we had the opportunity of seeing his mind actually at work on the theoretical problems raised by our recent discoveries; while in the laboratory we could daily see him pursuing his experimental researches surrounded by his vacuum tubes and pumps, and duly attended by the ever faithful Everett.

The ground floor of the original Cavendish laboratory consists of a series of intercommunicating stone-flagged rooms, each serving as a passage way to the next. Thus only in the innermost of all is any privacy possible. This last was the research room occupied by Maxwell, by Rayleigh, and later by Rutherford. For a time it was occupied by Thomson, and the electron was discovered there. But by the time I joined the Cavendish, Thomson had vacated it, and had established himself in the outermost room of all—the one through which all the busy traffic of the ground floor surged backwards and forwards. One could not find one's way to the workshop to petition for a fresh stick of sealing wax or a rubber stopper without passing through the Professor's research room. It is true that there was, on the first floor, a room furnished principally with a circle of hard wooden chairs, which was supposed to be the Professor's private room, but its main function was as a meeting place for the lab. teas—functions from which

the Professor himself was rarely absent. It was, in fact, known as "the tea room", and its door always stood open. No material obstacle ever separated Thomson from his students.

I am afraid that, with the normal impercipience of youth, we took a good deal of all this for granted, but subconsciously we must have felt it, and we responded with a respect which ripened into a deep and lasting affection. To us he was always "J. J.", and there was no one like him. Once a year, at the annual Cavendish dinner, we were allowed to overstep the normal bounds of decorum and to show him how much we thought of him. I think that in the full-throated chorus which always greeted the final toast of "J. J." he felt he had his reward.

In the matter of training physicists, as in experimental matters, "J. J.'s" methods were his own. The student learned his job by the slow and painful process of trial and error. There was no spoon-feeding and very little ready-made apparatus. The student was made to acquire the technique of his chosen profession before he could hope to produce results. As a means of obtaining experimental results the method was painfully slow, and the output of publications could easily have been doubled by a larger supply of ready-made equipment, and more continuous direction in the use of it. But it was Thomson's conviction that a university laboratory should be a training school for men, and not a factory for turning out results, and he steadfastly refused to use his research students as mere "observers". Thomson's methods might have had their dangers in the hands of a lesser man, but with him they were eminently successful. Our enthusiasm, fired by his, which was always at white heat, carried us safely over the stickier portions of the road, and there were few who fell by the way. Seven of the men who received the important part of their training from him have been awarded Nobel prizes, a record not likely to be equalled, while a constant stream passed out from the Cavendish, year by year, to fill the most important positions in many universities and research institutions, and to carry on in many lands the Cavendish tradition which they had learned from him.

In spite of the heavy burden of his research school, Thomson succeeded in maintaining his own research activities. His experimental work was now largely directed to an investigation of the positive rays in the discharge tube. His methods were as brilliant as ever, but, owing mainly to the inadequacy of the high-vacuum pumps of the time, the work proved unusually difficult, and it was only just before the last war that he succeeded in his object of weighing the individual atom. His theoretical interests still lay in the direction of an elucidation of the structure of matter. He was the first to arrive at the important conception of the atom as a system of shells of electrons, and to trace out some of the more important consequences of this hypothesis both in physics and in chemistry. The full solution escaped him, as it must have escaped anyone who had been brought up in the rigorous methods of Maxwell. There had been no perceptible falling-off in Thomson's amazing fertility in ideas, nor in the number of research students who flocked to the Cavendish up to the moment when the outbreak of war in 1914 brought everything to a standstill.

Thomson was elected President of the Royal Society in 1915, and in that capacity had the double task of persuading the Government of the contributions which science might make to the national cause, and of organizing and co-ordinating that contribution. For the latter task there were practically no precedents, as the application of pure science to technical and industrial problems on an appreciable scale had, at that time, hardly begun. In fact, to many the mere conception of it seemed to be at least as novel and daring as any of Thomson's other ideas. Results were so good that after the war the Department of Scientific and Industrial Research, in the founding of which Thomson played a leading part, was set up to encourage the use of scientists and scientific methods for the solution of peace-time problems.

In 1918 Thomson was appointed Master of Trinity College, Cambridge, but the delight of his old research students at this new honour for their Professor was turned to something like dismay when the announcement was followed shortly afterwards by the news that, in consequence, he had decided to relinquish the Cavendish professorship, which he had held with such distinction for 34 years. To have combined the duties of the Cavendish professorship, as he conceived of them, with the headship of a great college, would, however, clearly have been beyond the physical powers of any man, and he would not give to the Cavendish anything less than his best. He was appointed to an honorary professorship in Physics by the University, and accommodation for himself and a certain number of research students was assigned to him in the Rayleigh wing of the Cavendish Laboratory, which had been erected during his Cavendish professorship. He was succeeded in the Cavendish chair by the most brilliant of his pupils, Ernest Rutherford.

Thomson received the Nobel prize in 1906, and was knighted in 1908. It is strong proof of the wealth of affection in which he was held by his old students everywhere that even Court precedent and etiquette could not turn him into "Sir Joseph". "J. J." he always had been, and "Sir J. J." he became. The medals and diplomas bestowed on him by universities and other learned societies must have amounted, to use the words of a Cavendish poet, to "the load of a dray". His greatest delight was, however, in the successes gained, not by himself, but by his old research students, and the reward which he valued most was the loyal affection which he inspired in their hearts.

It is fitting here to record his connection with our own Society. He joined the Physical Society as a Life Fellow in 1895, and was President of the Society from 1914 to 1916. During his presidency his Presidential Address on "Ionization" and a few other papers appeared in the *Proceedings*, but for the most part he remained faithful to the *Philosophical Magazine* (of which he was an editor for many years) as a medium of publication. Thomson delivered the Thirteenth Guthrie Lecture in 1928, on "The Electrodeless Discharge through Gases". His last contribution to the *Proceedings* was a section of the obituary notice of the greatest of his pupils, Ernest, Lord Rutherford. Finally

there can be no harm in revealing now that the conception of our *Progress Reports* sprang from his fertile brain.

"J. J." was one of the wisest, kindliest and most unselfish of men. He had a wealth of erudition which he carried without ostentation. The wideness of his interests—co-extensive with humanity itself—coupled with his ready wit and infectious chuckle, made him a brilliant conversationalist. Few of us could be constrained by any other engagement to forego the daily tea parties in the laboratory, where he would "sit in the middle, and talk about everything under the sun". The gracious hospitality which he and Lady Thomson dispensed in their home in West Road, and later in the Lodge at Trinity, lingers in the memory of all who were privileged to partake of it.

To meet him walking down Trinity Street or Rose Crescent on his way to the lab., with the unseeing gaze and abstracted air of "one, for ever voyaging on strange seas of thought, alone", one might have thought him a typical example of the absent-minded professor, and Cambridge, which loves to weave legends about its great men, has many stories in which he figures in that rôle. As a matter of fact one learned by experience that very little escaped his notice, and there were few whose judgement of men and affairs was better worth seeking.

Of him it might truly be said, as was said of Faraday, "The man was even greater than his science. To read his scientific researches excited one's admiration : to know him warmed one's heart".

J. A. CROWTHER.

Sir ROBERT HADFIELD

Sir Robert Hadfield passed away on 30 September 1940 in his 82nd year. He was probably the best known metallurgist of his day and was a keen business man. Honours and distinctions came easily his way. During his long life he received practically every important medal awarded for metallurgical work. His two great contributions were manganese steel and "low hysteresis" steel. In 1878 his attention was attracted to alloy steels by a pamphlet describing the ferromanganese alloys of the Terre Noire Company, Paris. As a result of his investigations he found the method for heat-treating manganese steel, which resulted in a tough product being obtained by quenching. This steel has found wide application in conditions where resistance to wear is demanded, for example tramway wheels, tramway and railway crossings and rock-crushing machinery.

When only twenty years of age, Hadfield founded a laboratory at his father's steel works at Attercliffe. His early training was rather as a chemist than as a physicist. Chemistry at that time formed almost the sole basis of scientific ferrous metallurgy. There seems no doubt that the then surprising and remarkable non-magnetic character of his newly discovered manganese steel, as well as its very low conductivity for heat and other peculiar physical features, brought to him, through the interest shown by the prominent physicists of the day, including Kelvin and Dewar, a strong realization of the importance which the

physical properties of alloy steels might have, this both for the useful applications which it might bring them and for the better understanding of their nature. Physical as well as mechanical and chemical properties were thus always afterwards included in the examination of the various new alloys he made. Much of his work at that time, in the eighties and for some time afterwards, was carried out by collaboration between himself and prominent physicists, equipment with physical apparatus being then uncommon outside the universities and educational institutions. In 1925 he stated that during the past forty years or so more than 3000 different steels had been made and tested under his supervision.

What was probably the first physical laboratory in a steel works was set up at the Hecla Works early in 1902. Part of the functions of the physical laboratory was to take care of the pyrometers, which had then begun to occupy an established place in the manufacturing processes. The importance of accurate measurement of temperature had early been realized by Sir Robert, who purchased the first Le Chatelier electrical pyrometer in 1890.

Sir Robert was one of the first to notice and to realize the value of the discovery by Heusler that magnetic alloys could be prepared from non-magnetic metals. No doubt this appealed to him as being rather in the opposite sense to manganese steel, in which a magnetic metal, iron, had been rendered non-magnetic by alloying. He made and examined some of the Heusler alloys and exhibited them to the British Association in 1904. He published a number of papers on the electrical and magnetic properties of alloys in collaboration with Sir Wm. Barrett, Professor W. Brown, Sir James Dewar and Professor de Haas. Most of these papers appeared in the *Proceedings* of the Royal Society, the Royal Dublin Society, or the *Communications* from the University of Leiden.

He was happy when presiding over gatherings of scientists and technologists, and as president of the Faraday Society during the years 1914 to 1920 he was in the chair when the discussions on " Methods and Appliances for the Attainment of High Temperatures in the Laboratory " and " Pyrometers and Pyrometry " attracted large audiences in the midst of the Great War.

His inspiring influence had an effect extending far beyond the confines of these shores. Thus, for instance, on the occasion of the general discussion on the physical chemistry of steel-making, held in 1925, a series of papers were read which even in these days of rapid scientific advance stand as classical contributions to the development of the subject. The programme of research suggested by Sir Robert on that occasion was later drawn upon by the metallurgists of the U.S.A. in the development of an important co-operative research on steel-making carried out by the American steel-makers.

He devoted considerable attention and encouragement to the development of fuel technology, his works in Sheffield being amongst the first industrial organizations in this country to undertake the study of the subject. In his presidential address to the Society of British Gas Industries in 1918 the wide range of his interests was indicated by the many subjects to which he referred,

there being allusions not only to fuels, refractory materials and power, but to the hardness of materials, pyrometry, the encouragement of education, patents, and a proposal for a joint headquarters in London for our technical societies.

Hadfield also took great interest in the historical side, and made a detailed examination of the 79 specimens of steel which Faraday had prepared early last century. Faraday had prepared these steels with the object of finding the effect of adding various metals to steel. Hadfield's book ***Faraday and His Metallurgical Researches*** is a record of the work. Another of his books, ***Metallurgy and its Influence on Modern Progress***, is written in a popular style and is of absorbing interest even to the non-metallurgist.

Sir Robert was always thorough in his study of any subject he undertook. An interesting instance of his attention to detail is illustrated in the following incident. He had a large and very interesting collection of old metallurgical literature, and in order to add to the interest of a particular Faraday Society symposium he brought down a number of rare volumes to the meeting for the members to see. To use his own words: "In endeavouring to advance our knowledge in any particular science it is always profitable to look back and review its progress from the earliest beginnings". He followed this precept in many of his papers.

Anyone wishing to consult Hadfield's voluminous writings should refer to the pamphlet ***Classified List of Papers from*** 1888 ***to*** 1930, which comprises his scientific papers, technical papers and addresses. Obituaries dealing more fully with his activities will be found in *Engineering* for 4 October 1940, *The Engineer* of 11 October and *The Times* of 2 October 1940.

Sir Robert became a Fellow of the Physical Society in 1909 and served on the Council; in the same year he was elected a Fellow of the Royal Society. For 29 years he was chairman and managing director of Messrs. Hadfields, Ltd.

His departure from life's stage removes another of the group of pioneers who fifty years or so ago built up some of the great industries of this country.

In the preparation of this notice the writer has drawn upon some notes supplied to him by Dr. S. A. Main and Dr. R. J. Sarjant of Messrs. Hadfields, Ltd.

EZER GRIFFITHS.

ERNEST CUTHBERT ATKINSON

ERNEST CUTHBERT ATKINSON, M.A., F.R.A.S., who died on 3 December 1940, aged 67, was educated at Merchant Taylors' School and St. John's College, Oxford. He took 1st Classes in Mathematical Finals and Physics Finals, rowed in his College Eight, and was President of Oxford University Junior Scientific Club.

When at Oxford, he designed and made a "Rowing Indicator", which gave a continuous record of style and horse-power of an oarsman. This was exhibited at a Royal Society Conversazione and described in various journals

and books on rowing. He designed also the aluminium safety buffer used on the noses of Oxford racing boats.

He was for a time an Assistant Master for mathematics and physics at Rugby School and Curator there of the Temple Observatory; then an examiner in mechanics and physics at Oxford; and for a time on the Geodetic Survey of the Transvaal and Orange River Colonies.

On returning to England, he became an Assistant Master for mathematics at Clifton College in 1906. He resigned his mastership in 1912, but remained on the staff in an honorary capacity until 1922. There he took part in the musketry training of the College Engineer Corps and in coaching the college rowing, and was President of the College Natural History Society. He was also Secretary of the Bristol Radio Association and had begun taking Eiffel Tower time signals before the last war for checking the running of a grandfather clock with an improved pendulum.

During the last war he was for two years, first as Staff Sergeant, later as Lieutenant, of the Hythe School of Musketry, engaged in musketry instruction at various home camps, and then, for two years, an Inspector under the Director of Inspection of Gun Ammunition (Technical), Woolwich, visiting factories all round the country.

In November 1916 he worked out a mathematical solution for "The Drift of a Projectile in Direct Fire". Various appropriate military experts to whom he submitted it declined to examine it, some because it could not help the prosecution of the war and one because surely he could not have succeeded where so many had failed. In 1918–1919 a second attempt to get it examined also failed. But in April 1919 he was referred to one of a group of researchers at the N.P.L. who had been working at the problem. He reported on the work as in any case the pioneer work on the subject and, describing some parts of it as original in method and another as novel, said it would have been worth while to publish the results in 1916–1917.

In 1927 he took up the study of Precision Time-keeping, attracted thereto, in part at least, by Prof. R. A. Sampson's papers on the subject in the *Proceedings* of the Royal Society of Edinburgh. He constructed first an electrically operated pendulum with a gravity impulse lever of the Cottingham or Froment type, but with certain features of his own design. This is a seconds pendulum in a sealed glass cylinder. In 1931 he built a similar one, a half-second one, also in a sealed cylinder, but enclosed in a heat-insulated chamber with thermostatic control. All the operating mechanisms were made by him. The workmanship is very accurate and the finish beautiful. Heavier parts, e.g. castings for supports and bobs, were also tooled by him. Continuous observations of the time-keeping of the pendulum have led to successive improvements in design and improved time-keeping. The last in the seconds-pendulum series is No. 14. But he also used the pendulums in experiments to analyse and elucidate various fundamentals in pendulum theory, which he described in papers to the Physical

Society, viz.:—1930: "Escapement Errors of Pendulum Clocks"; 1936: "The Relation between Rate and Arc for a Free Pendulum"; 1938: "The Dissipation of Energy by a Pendulum swinging in Air" and "The Amplitude Deviation of Rate of a Pendulum: A second experiment". Other papers have been:—"On Airy's Disturbance Integrals" and "Knife Edge Supports for Pendulums". He had in preparation one on "Errors in the Comparison of Clocks with Radio Time Signals". His device for the observation of amplitude gave, he considered, much more accurate results than any other in use. His notes of details of construction, alterations, results, and the accompanying graphs form a valuable record of his researches.

He was in correspondence with researchers on time-keeping at Greenwich, Paris and Hamburg Observatories, and with others at the N.P.L. and in Belgium, France, Germany and U.S.A. His study of the writings of other workers was very careful and thorough, so that he was on various occasions able to point out errors to the authors, or to indicate shorter mathematical processes.

At Northwood, Middlesex, where he had resided with a brother since 1922, he was for three years secretary of the Rifle Club. For about 33 years he had been associated with the Notting Hill Rugby Clubs (the Rugby School Mission in London) and had attended the Club camp each year (except the four war years), including 1940. Many are the testimonies to his self-sacrificing devotion to the service of the Camps, where he was the doyen of the Staff, beloved by all. One friend says: "He was one of the most gentle and tender-hearted of men. He was compounded of cold science and deep humanity, and the product was very good."

H. W. A.

JOHN ARTHUR TOMKINS

JOHN ARTHUR TOMKINS, a Fellow of this Society since 1900, was born in 1874, and received his early training in London, where he was apprenticed to a firm of instrument makers. From 1892 to 1898 he was a student of Birkbeck College and the Royal College of Science. His first teaching post was at the West Ham Technical College, but in 1903 he was appointed to the staff of Bradford Technical College, where he remained until his retirement in 1939.

In the course of this long period Tomkins saw many developments at Bradford. In his earliest days the staff of the Physics department consisted of two full-time teachers only, and the hours of duty were very long, although his old colleagues assure me that the pace and strain of teaching were less onerous then than now. He managed the department single-handed during the last war. When peace returned, bringing with it floods of young men as students, great changes came with it. The new Education Acts, the retirement of the former Principal, the extension of the college activities, all had their own considerable effects. The staff was increased, hours of attendance were reduced, salaries were revised. Tomkins was unfortunate. The quantity of teaching demanded of him did not

diminish ; his administrative duties as lecturer-in-charge increased largely, and his salary was reduced, never to be raised again. These later years were a disappointment to him, and he tended to retire from some of his activities. For instance, the local branch of the Association of Teachers in Technical Institutions, which he had done much to found, lost his services. Nevertheless he never failed to maintain his active interest in his subject. A strong section of Applied Optics had by this time been added to the Physics department, and from time to time he published papers on this subject. Altogether, he appears to have published some 30 papers on various topics of physics and applied optics, a record whose significance may be gauged only by remembering that his normal week contained 20 hours of teaching and all the business connected with a complex set of day and evening classes run by the Physics department.

Through his long tenure of the lectureship he had been fortunate in his excellent health ; and it is all the more sad that he should have lived so short a time in retirement. There can be no doubt that Bradford has lost one of the most able and devoted of its former servants ; and though he was not a prominent participator in the activities of the Physical Society, his passing will bring real regret to the many Fellows who knew him. J. P. ANDREWS.

REVIEWS OF BOOKS

Four-Figure Tables with Mathematical Formulae, by H. C. PLUMMER, M.A., F.R.S. Pp. iv+88. (London: Macmillan and Co., Ltd., 1941.) 3*s*. 6*d*.

About half of this book is devoted to tables and half to a collection of formulae of elementary mathematics, the whole providing a useful *vade mecum* for the student.

The tables are in the main reprinted from those in Castle's *Four-Figure Tables*, but an additional page is provided, giving the logarithms of numbers from 1000 to 2000 at intervals of 1, and in some cases the mean differences have been adjusted differently. Cologarithms (colog n=log $1/n$) have also been added, so that, for example, ab/cd may be evaluated by adding four numbers. It may be noted that to make the quickest use of these tables, the cologs of c and d should be written down before the logs of a and b, so that the book is still open at the logarithm page, from which the answer will be obtained, as no antilogarithms are provided. Reciprocal functions are given in full detail since the sines, cosines, tangents, cotangents, secants and cosecants are all tabulated, as well as their logarithms (and, be it noted, their cologarithms, since log cos x=colog sec x). The table of degrees to radians, but not that of radians to degrees, is given, together with a table of squares, one of reciprocals, and one (up to x=6) of exponential functions.

The collection of formulae is almost entirely restricted to the traditional text-book subjects. Thus under "Algebra" we find the formulae for A.P. and G.P., the binomial theorem, and solutions of the quadratic and cubic equations. Other sections deal with plane and spherical trigonometry, where it is to be regretted that the methods suggested for solution of spherical triangles do not include any checks, and with plane and solid coordinate geometry. The section on calculus contains a good selection of indefinite integrals, of which the usefulness would be increased if, for example, the integral of $1/(a^2+x^2)$ were given instead of that of $1/(1+x^2)$. This section also gives the formulae for Simpson's rule for numerical integration, and deals with Fourier series. A section on "probability" (really on theory of errors) is a useful innovation in collections of this type, as is one on finite differences, where Simpson's rule appears again. The *Calculus* section ends with a short summary of methods for solving differential equations, and is followed by one on mensuration, where we find such varied information as that the area of a circle is πr^2, that its arc is $r\theta$ and that the angle between edge and face of a regular tetrahedron is $\sec^{-1}\sqrt{3}$.

In addition to the mathematical tables and mathematical formulae, there are three pages of well-chosen physical constants, including metric-to-British conversion factors, astronomical data, Rydberg's constant (to 8 significant figures, with its 4-figure logarithm) and Joule's Equivalent (to 3 figures, with its 4-figure logarithm). One or two points, however, might be reconsidered in the next edition. Thus the absolute zero is given as −273°·14 c.; the last figure is not known with certainty, but is probably more than 4. There is an entry—"Water: latent heat of fusion (0° c.) 80 cal./gm.; of vaporisation: 539 cal./gm." Since water can only freeze at or very close to 0° c., and can evaporate at any temperature, the omission of the statement that the last figure applies to 100° c. is serious. The reader might assume that the 0° c. applies to both figures which follow it. The density of mercury at 0° c. is not 13·596 gm./c.c., as stated, but 13·596 gm./ml.

Apart from these minor points, no errors have been noted, and the whole book will be very useful for general reference. The facts that it is well printed and strongly bound, and that the price is not high, will all commend it to the young student. J. H. A.

Elements of Acoustical Engineering, by HARRY F. OLSON. Pp. xv+344. (New York: D. Van Nostrand Co., Inc.; London: Chapman and Hall, Ltd., 1940.) 30*s*. net.

Progress in the art of sound reproduction has been materially assisted by the application to the design of acoustical instruments of the analogies between acoustical, mechanical and electrical systems, which lead to the concepts of acoustical and mechanical impedance. This book gives a very clear exposition of these principles, and discusses from this point of view a large number of the modern instruments employed in sound reproduction. Chief attention is paid to the two ends of the reproducing chain—the microphone and the loudspeaker. These are very fully treated. The principles underlying the design of most modern types are thoroughly explained, and their performance characteristics are described, the various types of directional characteristics obtainable being well illustrated with a large number of diagrams. A chapter is devoted to the usual methods of measuring the performance of such instruments. Comparatively little space is given to the field of sound recording and reproduction from records. The general methods employed in both disc and film processes are briefly outlined, but the only instruments dealt with are those used in reproduction from disc records. A chapter on architectural acoustics discusses various applications of sound-reproducing systems, and illustrates clearly the factors governing the choice of the most appropriate types of microphone and loudspeaker for particular purposes. The book closes with a short summary of the salient facts of speech, music and hearing.

The book is well and fully illustrated with some 200 figures, and brings together a mass of useful data, largely from recent publications, to which full references are given. It is clearly written, and can be thoroughly recommended to all engaged in the field of sound reproduction, whether concerned with the further development of the technique or solely with its applications. N. F.

A Text-book of Sound, by A. B. WOOD, 2nd edition. Pp. xvi+578. (London: G. Bell and Sons, Ltd., 1941.) 30*s*.

The subject of sound has jumped in two decades from the position of least importance among the subjects comprised within the domain of physics to one which—apart from electron theory and its applications—has probably the greatest number of text-books devoted to it. Whereas in 1926 there were but two experimental treatises in the English language of sufficient detail to cover an honours physics course or to inform the would-be research student of what had already been done, there are now over half a dozen such books together with many dealing with various specialized applications of acoustics. Dr. Wood's book has attained a well merited position of honour mid-way between these two classes and serves both the undergraduate student and the research worker, and is now presented in revised form with additions to bring it up to date. One can sympathize with the author when he says, in his second preface: "Since the first edition was published a large volume of literature has appeared relating to various aspects of the subject such as supersonics, hearing, noise, technical applications, etc., and some difficulty has been experienced in condensing this material into 'text-book' form", but the most assiduous delver into acoustical literature will have difficulty in detecting any important omissions among these branches of the science as well as in some others, such as edge-tones, not named in the sentence quoted.

Every serious student of sound will have Dr. Wood's first edition on his shelves; one cannot do more than advise him to replace it with the second. E. G. R.

Modern Theory of Solids, by F. Seitz. Pp. xv + 698. (New York and London: McGraw-Hill Publishing Co., Ltd., 1940.) 49*s*.

In the development of modern atomic theory and quantum mechanics, solids have repeatedly played a decisive rôle. To mention only some of the most important landmarks of this development:—

After the discovery of the quantum of action in black-body radiation, Einstein, by his treatment of the specific heat of solids, was the first to show that Planck's constant is also fundamental for our understanding of the structure of matter. Born's lattice theory in connection with Laue's discovery gave the first real insight into the structure of crystals. One of the great difficulties of the classical electron theory was the contribution of the electrons in a metal to its specific heat. It may be recalled that this question contributed towards leading Einstein to revise the foundation of classical statistics and to draw important conclusions on the wave nature of material particles. The problem was finally solved by Sommerfeld on the basis of the Fermi-Dirac statistics. Later, after the discovery of quantum mechanics, solids have again proved to be an extremely fruitful field for applications. Such fundamental features as the free passage of electrons through a lattice at low temperatures, the phenomenon of ferromagnetism and the cohesion of metals could be understood as due to typical effects of the new quantum mechanics.

Every student of physics should, of course, be well acquainted with these general features and should have a theoretical understanding of them. The field of applications has, however, grown rapidly in the last ten years. It is a credit to the fruitfulness of quantum mechanics that even applications of practical and industrial importance can now be made. The field has become so wide that it may be asked whether it is not now desirable to separate these applications, so important are they, from the main line of pure physics as it was at the time when electro-engineering was separated from physics. The reviewer holds that some such line of distinction could and should now be drawn.

Professor Seitz has undertaken to give, in a book of nearly 700 pages, a survey of almost the whole theoretical material at present available. The book is written with high precision and great care. There is no doubt that it will be very useful to the specialist, who is interested in all the details of the theory and its applications. He will derive many suggestions for further research from it.

Some parts of the book are, however, open to criticism. It seems that there is some misunderstanding as to the true aim of a physical theory, if applied to systems which are too complicated to be treated exactly. What is to be expected of a *good* theory is that it shall allow one to understand the main features in a simple way. The Einstein-Debye theory of the specific heat of solids is an example. No simple theory will, however, reproduce the behaviour of such complicated bodies as solids with high accuracy. It may, of course, be desirable in many cases to improve the calculations later on, with the aim of higher accuracy, and at the cost of simplicity and clarity, but it should be made clear that these are refinements and not the primary aim of a theory of solids. In the book under review there is, for instance, a chapter on molecular binding and another one on metallic cohesion (the author's own research). The former begins with the most accurate calculations of the binding energy of H_2, and the latter with the latest refined version of Wigner and Seitz's theory. The reviewer feels that a reader who wants to learn why metals keep together will obtain a clearer insight into this beautiful theory from reading the author's original papers than from this section of his book.

The book begins with a chapter on the classification of solids, including a large

number of both simple and complicated experimental results. It is followed by a discussion of Born's lattice theory, the specific heat of solids and the behaviour of the electrons in metals treated as free. Together with Chapter XV, dealing with the conductivity of metals, these sections are the best of the book. Simple theories and general ideas are discussed first, followed by a very good representation of the more detailed theories and further applications. After an introduction to quantum mechanics and the many-body problem, the author proceeds to discuss molecular binding and Bloch's theory. This latter chapter is the very foundation of the modern electron theory of metals. The author discusses in detail the band structure and the Brillouin zones for various types of lattices. The basis of the whole theory is the fact that the wave function of an electron in a metal is a periodic function superposed on a plane wave. This fact is proved for special simple lattices. In view of its fundamental importance it would have been more desirable to give Bloch's own derivation, especially since this is extremely simple. Apart from the chapters on metallic cohesion (see above), the rest of the book is devoted to the more special properties of solids—the work function, excited electronic states, optical properties of metals and magnetism. These effects are all adequately dealt with, only ferro-magnetism is perhaps treated too briefly. A special chapter deals with phase transitions. A number of phenomena are described and some theoretical derivations are given, but it is difficult to discover a general line in this chapter.

Although the reviewer feels that only some parts of the book can be recommended to the student, it must, on the whole, be welcomed. It will certainly be useful for research workers in many branches, for whom it provides an excellent survey of the whole subject. The book is well produced and there are numerous graphs and diagrams.

W. H.

Spectrochemical Abstracts, Vol. II, 1938–9. By E. H. S. VAN SOMEREN. Pp. 38. (London: Adam Hilger, Ltd., 1941.) 5*s*. post free.

So extensively are spectrographic methods being applied to the analysis of materials that it is not surprising to find volume I of these abstracts, for which Mr. F. Twyman was responsible, and which covered the period 1933–7, being already followed by volume II; this covers the succeeding two years and includes also a few important papers published earlier, as well as books published in 1940.

The usefulness of both volumes is enhanced by the fact that the abstracts in the second are numbered consecutively with those of the first, the arrangement of which is followed with minor variations. The first half is occupied by a list of references arranged alphabetically under the authors' names, and the second half by 165 classified abstracts of papers and notes on books, reviews, charts and tables. Some idea of the large amount of work done in the short period covered by the present volume may be gained from the classification of abstracts according to the substances analysed: 48 abstracted papers deal with metals and alloys, 16 with biological material, 16 with non-metallic solids, 10 with gases and liquids and 7 with glass and pottery. Apparatus for such work is described in about 50 of the papers dealt with in this volume, technique in about 40 and theory and principles in about 20.

While opinions differ as to the length desirable in an abstract, it is fairly generally held that it should not run to such a length as to render a study of the original paper unnecessary (excepting, possibly, a paper in a journal to which access is difficult or

impossible), but should be full enough to indicate the scope of the investigation described. Most of the present abstracts fulfil this requirement, but some are perhaps unnecessarily short. Thus, in the case of one paper known to the reviewer, the investigation was carried out with meticulous care, but the abstract is so brief that it cannot but convey the impression that the work was either ordinary or perhaps even careless.

Nevertheless the booklet will prove to be extremely useful to all engaged in this work, and it is to be hoped that further progress in the subject will call for an early volume III. W. J.

The Special Theory of Relativity, by HERBERT DINGLE. (*Methuen's Monographs on Physical Subjects.*) Pp. vii+94. (London: Methuen and Co., Ltd., 1940.) 3*s.* 6*d.* net.

In the preface to this monograph on the *Special Theory of Relativity*, Professor Dingle offers an apology for writing a book on a subject about which so much has already been written and said. But he is a physicist who has learned and thought much about his subject, and as a result he has felt stimulated to write about it. For this sequence of events, which stands in favourable contrast to the reverse, little apology is needed, especially as the author has something to say on the subject which has not been said before, and new ways of putting much that is already accepted.

The book is a plea for the application of well known and well tried processes in Physics in the presentation of the theory of Relativity. The author believes in the statement once made by the founder of the theory that the Principle of Relativity is Physics. This might indeed be taken as the text of the monograph.

The history of the development of the subject has tended to a view expressed in an extreme form by Minkowski, who, when he realized that a four-dimensional continuum provided a convenient geometrical conception of time and space, expressed himself in a way that suggests a belief that something had been done which altered the nature of space and time. This is an expression of enthusiasm, and doubtless also of delight, which may be forgiven in one who was a pioneer in a series of exciting discoveries.

The author takes the view that results of experiment suggested, and even demanded, a new method of description, and he bases his argument largely on the empirical suggestion that the conception of length required modification. The physically important quantity is not l as it was previously defined, but a fraction f of l. The value of the fraction has to be sought in experiment and turns out to be dependent on a ratio identified with v/c, where v is the velocity of the body measured and is relative to any standard of rest which may be chosen. With the adoption of a new unit of length, a modification of the unit of time is a necessary consequence, and in this connection it is pointed out that nothing happens to a clock to alter its rate simply because we have altered our notions about what time-intervals are physically significant.

The Lorentz formulae are deduced on the basis of these modifications of the units of length and time, and we then pass to a number of fundamental formulae.

There is no attempt to cover a wide scope; the author is content to establish the velocity relations, the dependence of mass on velocity and some other relations of relativistic mechanics and of electromagnetism. His object has been to lay stress on the physical concepts of the theory. This is why the book should be read by students of Physics and by others interested in the theory who may have followed its historical development and, in doing so, have been attracted by its mathematical character and scope, overlooking its purely physical aspect.

The theory of relativity and its contemporary, the quantum theory, have succeeded brilliantly in a quantitative description of the physical world, but there are already indications that new ideas are wanting for further advance. These theories may share the experience of their predecessors but, whatever the future may reveal about them, they will be remarkable for the light shed upon the modes of thought and of description in Physics. The principles for which Dingle's book stands are sound in that by them the student is taught to see description in its true light as something which reveals but does not alter the content of what is described, and he will be the more ready to leave the limitations of an old for the freedom of a new system. H. T. F.

French-English Science Dictionary, by LOUIS DE VRIES. Pp. viii + 546. (London: McGraw-Hill Publishing Co., Ltd., 1940.) 24*s.* 6*d.*

The plan of this French-English dictionary is similar to that of the German-English one by the same author. Both are beautifully printed and bound, and both, catering nominally for scientists, show a tendency to favour words from the biological sciences over those from physical or engineering science. Intentionally, and wisely, they also include many words of general use (*fable*, *fauteuil*, *lire* and *penser*, for example), but it is a matter of doubt whether these need include *le*, *la*, *les*, *il*, *ils*, *elle* and *elles*; and if *armistice*, why not *trêve*? Prepositions, which are often troublesome in a foreign language, are fairly fully treated, but the meaning "out of" has been omitted from *dans* ("Il prend une cuiller *dans* le tiroir, avant de boire *dans* la tasse"), and "from" is not included among the meanings of *avec* ("Le divorce *avec* la vérité . . . ").

The author has considered it desirable to include many words (they must run to hundreds or even thousands), which are practically identical in the two languages (inductance, indigotin(e), indican, médoc, nigrite, néon, opaque, électrode, mannitan(e), mannose), on the ground that the student may then consult an English dictionary for further information.

One would naturally turn to a scientific dictionary to resolve the doubt as to the precise significance of *limon* and *citron* in French, but no help is given here. The meanings of both are said to include "citron" and "lemon". This does not agree with the 1295-page *Dictionnaire* of Littré and Beaujean, which does not give either as a synonym of the other. The various shellfish are always rather confusing, and whilst *langouste* is here translated as "spiny lobster", *écrevisse* is said to mean "crawfish, crayfish, crab, lobster . . ."; *homard* is also "lobster", *crabe* is "crab" and so is *cancre*. Possibly these are accurate, but surely the use of *écrevisse* for crab and lobster is so rare that these meanings might be so marked, especially in a scientific dictionary, where the distinctions may be of importance? (Is it perhaps *écrevisse der mer* which means lobster, as one dictionary states?)

Of other words which have caught the reviewer's eye, the equivalents "to waver, to hesitate" are omitted from *balancer*, *cocotte* is mentioned as foot-and-mouth disease, but not as a stewpan, and the word *pique-feu*, unknown to the reviewer or to Littré and Beaujean, is given for a poker. *Repondre*, "to lay (egg) again", seems to apply to a very rare process. *Fanal* is translated simply as "lantern", though one common meaning is that of a railway locomotive light, and *phare*, given as "lighthouse, beacon, headlight", would be the better for a definite reference to motor-car headlights. Under *repère*, the English technical word for "point de repère" is "fiducial mark". Since nearly

a dozen inks are mentioned, the omission of *encre de Chine* (Indian ink) is a little surprising.

There is a very useful collection of abbreviations at the end of the book, where one (quite trivial) misprint has been noted: under C.Q.F.D., the word *démonstrer* should be *démontrer*. These abbreviations include many of ordinary life (P.T.T., for example), and, since *chômer*, "to be stood off from work", and *grève*, a strike, are in the dictionary, it might have been worth while to add C.G.T. ("Compagnie Générale de Travail", the French T.U.C.).

As will be seen from the above, the book, with its 43,000 entries, does not suffer from undue exclusiveness of choice, and is to be highly recommended to those seeking a one-volume French scientific dictionary. J. H. A.

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Radio Interference from Thermostats in Refrigerators and Irons, by S. F. PEARCE and S. WHITEHEAD. (Technical Report M/T67, 1941.) THE BRITISH ELECTRICAL AND ALLIED INDUSTRIES RESEARCH ASSOCIATION, 15 Savoy Street, London, W.C. 2. 4*s*.

The High-Frequency Properties of Various Forms of Wire Specimens, by G. G. SUTTON. (Technical Report M/T69, 1941.) THE BRITISH ELECTRICAL AND ALLIED INDUSTRIES RESEARCH ASSOCIATION, 15 Savoy Street, London, W.C. 2. 6*s*.

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